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Illustrations OF HOSPITAL PRACTICE:

METROPOLITAN AND PROVINCIAL.

HOSPITAL FOR SICK CHILDREN.

FATTY TUMOUR CONNECTED WITH THE INTERIOR OF THE SPINAL CANAL: OPERATION.

Under the care of ATHOL JOHNSON, Esq.

THE patient was a male child, aged 10 months. It was born, according to the mother's account, with an ulcer over the sacrum, but no perceptible swelling below the ulcer. At three weeks old, it was brought as an out-patient to the hospital, and put under the care of Mr. Johnson. There was then an ulcer, of about the size of a half-crown, over the sacrum, rather to the right of the middle line. Beneath this was a slight ill defined swelling, and by its side a small pendulous excrescence of fat, covered with integument. The ulcer gradually healed, and has remained soundly cicatrised. The swelling beneath it, however, slowly but steadily increased in size; but without being attended with any inconvenience to the child, whose general health was excellent until the beginning of January. At that time the swelling enlarged rapidly, and the child suffered from repeated twitchings, or convulsive movements, affecting the *right* leg only. This made the parents anxious to have something done to remove the swelling. A grooved needle was introduced, but no fluid was found.

Feb. 3. After consultation, it was determined to operate. At this time the swelling was of about the size of a small orange, but broad and ill defined at its base. No separate tumour could be distinctly felt; but underneath a soft doughy mass there seemed some more defined hardness. It was seated over the sacrum, rather to the right of the middle line. There was no tenderness, nor any inflammation of the skin. The pendulous excrescence, above referred to, was of about the size of a large pea. The child's general health remained good.

Chloroform having been administered, an incision was made through the swelling into the midst of a mass of subcutaneous fat, not circumscribed, nor contained in any distinct capsule. After cutting through this, to the depth of more than an inch, and removing as much of it as was necessary, the smooth and glistening surface of a capsule surrounding a distinct fatty mass, which constituted a separate tumour, was found; and, on proceeding to remove it, it was found to adhere to the parts beneath, requiring careful dissection. After removing it, an aperture, large enough to admit the end of the finger, was seen in the posterior wall of the sacral canal, through which the tumour seemed to have made its way. Rising up to the level of this opening was a soft mass, apparently formed by the membranes of the cord, and which exhibited a very distinct pulsatory movement, visible to the eye, especially when the child cried, as it now began to do, having partially recovered from the effects of the chloroform. The tumour seemed to have been partially adherent to these membranes, from which it was dissected off (a little granular fat, however, being left adhering), without any opening having been made in them. The parts were then brought together with sutures, and a compress and bandage applied.

Six days after the operation (Feb. 9th), the child had gone on without a bad symptom, and a considerable part of the wound had healed by the first intention. The mother stated that there had been no convulsive movements of the leg since the operation.

It may be mentioned, that the father of this child had had a fatty tumour removed from his back some time since.

REMARKS. The singular case of which we have given an account has no exact counterpart, as far as our observation has extended, in the records of surgery. No such complication is mentioned in numerous authors on the subject of spina bifida, whose works we have looked over. The nearest approach to it is a case reported in the *Transactions of the Pathological Society*, 1851-2, p. 248, by Mr. Obré, in which a collection of fat was found in the spinal canal. The subject, a child, had, when a year old, suffered from partial paralysis of the right arm and leg, following after convulsions. The symptoms had disappeared, without obvious cause; and the convulsions recurred at the age of three years. The child died before being seen by Mr. Obré. At the *post mortem* examination, a deposit of fat was found in the upper part of the spinal canal, lying between the theca vertebralis and the bodies of the last cervical and first dorsal vertebræ. It was two and a half inches in length, about half an inch in thickness, and occupied about the whole breadth of the canal. The fat was more solid and granular than usual, and this, it may be mentioned, appeared to be the case with the small portion of fat left on the membranes in the case above reported. The membranes of the brain and cord above the disease were congested. This case, although it agrees with Mr. Johnson's in the particular of the existence of a deposit of fat within the canal, differs from it in that which gives the latter its peculiar surgical interest; viz., that that deposit coexisted with the deficiency of the bony canal necessary for the formation of a spina bifida, and occupied the opening in that canal, thus rendering it hopeless to establish an exact diagnosis; for, besides the situation of the tumour, which would probably in any case have prevented the opening in the bones from being made out, the depth at which it was situated below the abundant subcutaneous fat rendered this wholly impossible. That the parts exposed by the removal of the tumour were the spinal membranes, raised up by, and receiving the pulsation of, the subarachnoid fluid, admits of no doubt. This pulsation is very minutely described by Magendie, *Recherches sur le liquide céphalo-rachidien*, Paris, 1842, pp. 40 *et seq.*, as being synchronous with the respiration. At p. 44, he says: "At the time of expiration, when the thorax contracts by its own elasticity, and the forces of expiration drive the air out of the lungs, the arterial and venous trunks of the thorax are compressed, and the venous blood flows back towards the organs. This reflux is particularly sensible and rapid in the intra-rachidian venous plexus, which, from its proximity, feels the first effects of the repulsion of the blood. Thus disturbed, this plexus itself exerts compression on the subarachnoid fluid, which mounts towards the cerebral cavity." And in another place, p. 41, he says that he has observed, in cases of spina bifida, that the pouch is distended during expiration, and collapses partially in inspiration. Cruveilhier also (*Anat. Descriptive*, 1836, vol. iv, p. 552) describes, from experiments made on a dog, a double movement in the subarachnoid fluid, one synchronous with the pulse, and another with the respiration. The latter was the only pulsation distinctly perceived in the case above described; though some of those who assisted at the operation believed that the other was also present. If so, however, it was very slight; and, as the parts were left exposed only for a very short time, it was not possible to come to a distinct conclusion on this point. It may, however, be doubted whether at that part of the canal this pulsation, which must be derived there, if it exist, solely from the arteries ramifying in the membranes,* could be perceptible, especially in so young a subject. (T. H.)

* For the wave of pulsation in the fluid above would surely be insufficient to produce motion.

ST. GEORGE'S HOSPITAL.

IMPERFORATE VAGINA: PUNCTURE: PERITONITIS.

Under the care of R. LEE, M.D., and T. TATUM, Esq.

MARY F., aged 18, was admitted on November 5th, under the care of Dr. Lee, on account of imperforate vagina. She had been in the hospital before for several weeks, in the early part of the year, when, as there were no urgent symptoms, it was decided to abstain from operation. At the time of her present admission, there was considerable swelling of the lower part of the abdomen (which had increased much since her previous stay in the house), with distinct fluctuation. The vagina was closed, about three inches from the external parts, by a septum or band, which entirely cut off all communication with the os uteri. She had therefore never menstruated, but for about a year had suffered pain at the monthly periods, and had noticed a gradual enlargement of the hypogastrium. Examination *per rectum* showed the uterus much distended with fluid. Latterly these pains had been getting much worse, and recurred more frequently (about every fortnight), sometimes accompanied by severe fits, probably of hysteria. There was no pain on pressure. The sensation of fluctuation was easily communicated through the vaginal septum by pressure on the parietes of the abdomen. She was kept in the house some time, in order that the case might be watched; and on December 2nd, as the swelling was steadily increasing, and the pain becoming more constant and more severe, Mr. Tatum's assistance was requested; and the septum was perforated with a trocar. This allowed the escape of about six ounces of dark brown treacly fluid, which was evidently accumulated menstrual secretion. She complained of great pain at the time, and for several hours afterwards, for which a course of calomel and opium was prescribed. Similar fluid continued to drain away for the rest of the day. She slept well with morphia, and complained of no pain till next evening, when she was suddenly seized with intense pain in the abdomen, aggravated by pressure. The discharge had ceased. The pulse was quick, irregular, and irritable; the thirst great. The mercury was continued, and leeches applied to the abdomen, which was assiduously fomented. A large dose of morphia was given. The pains after this became more intermittent, and appeared to be relieved by the occasional reappearance of the discharge. For the next few days, however, she remained in a rather critical state, vomiting frequently, with very low pulse, and great tenderness on pressure. A week after the operation, the dark coloured discharge ceased, and was replaced by one of a leucorrhœal character. On the sixth day after the puncture, the discharge was extremely copious, and very offensive; as much as three or four pints were computed to have come away. The pains for the next few days were severe, but intermittent, and were referred to contractions of the uterus, and not to peritoneal inflammation.

She remained in the house till the end of January, and during that time had one access of the catamenia, as was supposed. The menstrual discharge, however, was but scanty. The opening made by the trocar remained patulous, and the uterus was found to have recovered its proper size.

REMARKS. Cases of imperforate vagina are rarely seen in our hospitals, although many instances of this deformity are recorded by various authors. The case cited above is the only one remembered to have occurred at St. George's during a period of several years. Some years ago, a similar case occurred under the care of Mr. Cutler, in a young child about seven years of age, in which Mr. Cutler succeeded by careful dissection in finding the os uteri, and, by means of a sponge-tent, preserving the canal of the vagina during the healing of the wound. In a child, however, before menstruation has commenced, such a proceeding is much more feasible than in a case like the above, where the uterus is distended by a large accumulation of the retained menses, where the irritation may have spread to neighbouring parts, and where the mere exposure to the air of such a mass of fluid occupying the cavity of the uterus may have the effect of setting up a dangerous inflammation. It was probably under the influence of some such considerations that, on the occasion of this young woman's former stay in the hospital, her medical attendants, after consultation, decided on not attempting anything while no more urgent symptoms were present; and it was not till the persistence and steady increase of the accumulation, and of the consequent pain, showed that something must be done to relieve her, that surgical interference was permitted. It was then limited to the most prudent extent; for not only is there great risk from the mere fact of making an opening—a risk which, in the given circumstances, must necessarily be incurred—but it is not easy to feel confident that, in so unnatural a state of parts, some further deformity may not exist; that, instead of the vagina being merely closed by a septum, that tube may not have been partially deficient, and even that the peritoneum may not be brought lower down than in the natural condition. In either case, great mischief may be done by a free incision: in the former, such an incision would lead into the deep cellular tissue of the pelvis, into which the fluid evacuated from the uterus would thus be introduced, leading to the subperitoneal inflammation so often met with after operations about the pelvis; and, in the latter, this fluid would be effused directly into the peritoneal cavity itself. It is not without reason, accordingly, that the best writers (Dr. Davis's *Obstetric Medicine*, vol. i, 110) recommend caution in dealing with cases of this kind—a recommendation which the history of such operations abundantly justifies. Instead, therefore, of the free incision which would have been made into an imperforate hymen, this case was treated merely by the puncture of a trocar; yet the sequel showed that the dangers even of such an operation are not slight; and, after her recovery from it (which seemed at one time precarious), her attendants were fain to be content with having left a passage to the os uteri, without attempting to enlarge that passage by any mechanical means to the calibre of the rest of the tube. In fact, this dilatation is not absolutely essential. Fecundation is always possible where there is a passage, however small, to the uterus; and many cases are on record (in the work referred to above, for instance) in which parturition has been safely effected, notwithstanding partial constriction of the vagina. It seemed better, therefore, not to expose the patient to a risk which was certain and imminent, in order to avoid a subsequent and uncertain danger, the results of which might after all prove trifling. (T. H.)

UNIVERSITY COLLEGE HOSPITAL.

I. LOCAL TREATMENT OF CANCER: APPLICATION OF THE SULPHATE OF ZINC AS RECOMMENDED BY DR. SIMPSON.

Under the care of J. ERICHSEN, Esq.

SULPHATE of zinc has been applied in two cases at the above Hospital, for the opportunity of observing which, we are indebted to the courtesy of Mr. Erichsen. One was a man, who had been admitted with an ulcer of the leg, rendered intractable by a mass of fungous unhealthy granulations connected apparently with the periosteum of the tibia. These had been destroyed by the application of the sulphate of zinc paste, and the ulcer was healing kindly. Of the other, a case of epithelial cancer of the clitoris and inguinal glands, we subjoin a few notes supplied by Mr. Tevan, the house-surgeon. In both cases the application of the escharotic appeared to have been followed by pain, acute for the first few hours, and lasting (at least in the man's case) for more than a day. No other bad effect, however, had followed; and the caustic appeared not to have acted on the healthy tissues in the neighbourhood. A minor advantage of this substance is that, as it acts only on parts divested of cuticle, it may be freely handled by the operator without any risk of "burning his own fingers."

The case which we reported in our previous number from St. Bartholomew's, as well as the other to which we alluded, as being under treatment with Burnett's solution diluted, are progressing very favourably, but are not yet sufficiently advanced to render it advisable to publish more details at present. We have thought it worth while, however, to give the notes of a few cases, although as yet imperfect, in order to induce those of our readers who have opportunity and inclination to test some of the plans now under trial, and shall esteem it a favour to be furnished with the results of any such cases.

Hannah W., aged 58, was admitted Jan. 10th, under the care of Mr. Erichsen. She was married, but had had no children. She seems to have had irritation and ulceration inside the labia for a great many years, but no distinct tumour till some time ago (unknown, but less than two years). About a month ago, she noticed a swelling in the right groin, which increased rapidly, and soon became painful. On admission, a raw ulcerated surface, with very hard base, was found inside each labium, extending upwards to the symphysis, and downwards into the vagina. It was very tender, and there was profuse discharge. There was also a large mass of malignant disease in the right groin, and this also was very tender to pressure. There were marks of old sores about the thighs. She was in bad health, and had been insane about a year before. Slept

badly. A few days after admission, an abscess presented in the right groin, and was opened. As some suspicion of syphilis was entertained, the pus from this abscess was inoculated on the thigh, but did not produce a chancre.

Jan. 21st. She was brought into the theatre, and an attempt was made to narcotise her with amylene. This, however, could not be done, though she inhaled it for about a quarter of an hour. The extent of the disease rendering it impossible to remove it with the knife, the caustic recommended by Dr. Simpson, viz., an ounce of sulphate of zinc (previously carefully dried) made into a paste with a drachm of glycerine, was applied. This produced severe pain for a few hours, but on the sloughs caused by it separating, which they did five days after, the surface exposed was healthier. The patient also expressed herself somewhat relieved from the pain, and slept better.

Feb. 4th. The application of the caustic was repeated.

Feb. 6th. When last seen, the parts were covered by a dense, dry slough. She expressed herself as being much relieved, better in health and spirits, and the disease had certainly not extended in the deeper parts.

II. PLASTER OF PARIS BANDAGE.

We had an opportunity of seeing the application of this bandage at University College Hospital lately. It is intended as a substitute for the starch bandage in ordinary use at present, the advantages attributed to it being that it dries much more quickly, and is lighter. The former quality causes it, as is said, to sit more accurately, as the patient finds it easier to maintain the same position during the shorter period, so that it is free from the creases which are apt to be produced in the starch bandage by involuntary movements; and the patient is ready to be moved much sooner than when the starch is used. This is a point sometimes of importance in military practice. The bandage was applied at Mr. Erichsen's desire by Mr. Gamgee, who had seen it used abroad. It is said to have been in much favour with Pirogoff at Sebastopol. It is made by rubbing dry plaster of Paris well into an ordinary bandage; a number thus prepared can be kept ready for use. The patient's limb is first enveloped in a roller, and over this the plaster of Paris bandage is applied, slightly wetted (to prevent the plaster from shaking off.) As each turn is laid on it is more thoroughly moistened, and then lies quite evenly and comfortably. At any part where more support is required, additional strips of the same can be laid on and supported by turns of the principal bandage. The patient was seen two days after the application. The apparatus was described as very comfortable, and appeared to answer, in all respects, as well as the ordinary bandage. (T. H.)

ST. GEORGE'S HOSPITAL.

THREE CASES OF HEPATIC ABSCESS.

Communicated by G. GODDARD ROGERS, M.D., Medical Registrar to the Hospital.

CASE I. Under the care of H. Bence Jones, M.D., F.R.S. G. E., aged 58, an emaciated person, who worked as a porter in some establishment in Bond Street, was admitted on 31st August, 1855, with dyspnœa, livid countenance, and severe pain below the right costal cartilages. He stated that his health had been very good until about five weeks ago, when he suddenly felt acute pain at the pit of the stomach and between the shoulders. At this time, he noticed short hacking cough, but he did not expectorate; and his breathing was not at all affected until a week before admission. He had been carefully watched by an eminent physician in the neighbourhood, who applied various counterirritants to the epigastrium, and treated him likewise constitutionally; but his circumstances did not allow him the necessary conveniences for a protracted illness, and he therefore left his medical adviser, after being three weeks under treatment. Ether and ammonia were given, and he was well purged. On the following day, tincture of bark was added to the mixture. He expressed himself as feeling much relieved, took his food well, and passed a fair quantity of healthy urine.

Sept. 2nd. This morning, whilst on the night-stool, he had a severe rigor, which lasted three quarters of an hour. At night, he shivered again, but the attack was milder. During each of the paroxysms, his body became of a red colour, giving him the appearance of a person suffering from scarlatina. Throughout the day, his tongue was much coated. Some brandy revived him, and for three days he had no return of the shivering.

Sept. 6th. During the night he had slight chilliness, and in the morning a severe rigor, which lasted half an hour. He expressed himself as feeling much worse. His pulse was feeble and rapid; the palms of his hands hot and dry; tongue thickly coated with brown fur; and all his body was of a deep red hue. Some calomel and Dover's powder were prescribed; and as towards evening he complained of much abdominal uneasiness, a dose of castor oil was ordered for the morning of the 7th. Throughout the night, he was very restless, and three distinct rigors attacked him.

Sept. 8th. The gums were somewhat tender from the mercury. He was rapidly approaching a typhoid condition. His lips were blue and skin very cold.

On the morning of the 10th, erysipelas appeared over the face, and no doubt hastened his end, as he sank at 6 P.M.

POST MORTEM EXAMINATION eighteen hours afterwards. A slight amount of bloody fluid existed in both pleural sacs and in the pericardium. The bronchial lining membrane was vascular. In the apex of the left lung was a small amount of scrofulous matter. The heart was soft, and the right ventricle rather thin. On raising the liver, the duodenum was found adherent to it by its middle portion. At this point the duodenum presented internally a large ulcerated opening admitting the little finger, and communicating with an abscess occupying the position of the gall-bladder, *of which no trace existed.* The abscess also penetrated into the lobulus Spigelii, and its entire contents were dark ash-coloured matter. Nothing like a biliary calculus was found, though probably the cystic duct and gall-bladder had been destroyed by ulceration from arrested gall-stones at some previous period. The lining membrane of many branches of the portal vein was coated by fibrinous deposit of a reddish-brown colour.

CASE II. Under the care of H. Bence Jones, M.D., F.R.S. T. J., aged 48, a stableman, of intemperate habits, and sallow complexion, had been an out-patient of the hospital for some time for bronchitis. He applied for admission into the house on the 14th of March, 1855, on account of severe paroxysms of pain in the region of the liver. There was also dull aching pain in the loins, and his urine was scanty and slightly albuminous. With the exception of a few moist râles, there was nothing much amiss with the breathing. He was well purged, and ordered a demulcent mixture. For the first ten or twelve days there was very little change in his symptoms.

March 28th. He experienced about this date much gnawing pain at the epigastrium, and he was unable to lie down. His appearance was more cachectic; and he had harsh dry cough. Sinapisms and opium liniments were successively tried; but he was not at all benefited. At times he was bent double with the pain. Opium in its various preparations was given, and he began to feel easier again.

April 14th. Vomiting was now an urgent symptom. His countenance was sunken and cadaverous. The epigastric pain was not so severe; but the chief uneasiness was in the right hypochondrium. A blister was laid across this part, and seemed to give great relief. He had very little vomiting until the 10th of May. Then it again troubled him; and as he had been daily getting weaker he was soon prostrated by the sickness. The abdominal pain was more widely spread, although the old spot (the hepatic region) was most tender upon pressure. He became unable to converse, and sank in about a fortnight.

The examination was made twenty-nine hours after death. The heart and lungs were quite healthy. There was a large abscess in the back part of the right lobe of the liver which had opened into the peritoneal cavity; but the adhesions contracted between it and the ascending colon had circumscribed the purulent effusion. Several very large firm masses of encephaloid deposit were disseminated throughout the substance of the organ. The gall-bladder was of enormous size, and full of bile. The mesenteric glands to some extent were infiltrated with encephaloid matter.

CASE III. Under the care of R. Nairne, M.D. M. W., aged 24, was under a surgeon's care on first coming to the Hospital, but was, on the 6th April 1855, placed in a medical ward owing to a feverish attack, coupled with pain in the chest, headache, and uneasiness across the loins. About a week later, cough came on, with dyspepsia, and tenderness over the right loin. She was treated with saline febrifuge draughts, Dover's powder at night, and counterirritation to the chest and loins. There was some pneumonia on the left side at this time, from which she soon recovered. On the 18th, her as-

pect changed in a remarkable manner to the depressed look of fever. She complained of pain over the cæcum, and the bowels were relaxed. Wine, and starch and opium injections were ordered. This depressed and listless appearance increased, and profuse perspiration with vomiting and abdominal tenderness came on. On the 23rd, she had several rigors, and complained of acute pain in the right hip and thigh. The urine was scanty and albuminous. Every day up to the 3rd of May, the shiverings returned at uncertain intervals. The succeeding perspirations were most profuse, and gradually wore her down to extreme emaciation before death took place.

At the examination, which was made twenty-two hours afterwards, the lower part of the left lung was found hepatised. A considerable quantity of yellow fluid existed in the peritoneal cavity. The right lobe of the liver contained a very large abscess, and the right kidney was adherent to its under surface, so that on separating the two organs the abscess broke. Another abscess existed in the same kidney, which probably bore some relation to that in the liver, but the connexion could not be clearly traced. The left kidney was large and rather softened. All the other organs were natural.

Remarks. The most interesting feature in these cases is the total absence of ulceration or other unhealthy appearance in the intestines; unless, indeed, we look upon Case II as one of abscess of the liver consequent upon ulceration of the duodenum. I am, however, of opinion that the correct view is that taken in the account of the autopsy, which attributes the lesion of the intestine to arrested gall-stones. This explanation is to some extent strengthened by the statement of Mr. Curling (*Med.-Chir. Trans.*, 1842) founded upon careful examination after deaths from burning. He considers that abscess of the liver never occurs as the result of ulceration of the duodenum in these cases.

Annesley looked upon pain in the right shoulder as a tolerably sure indication of suppurative inflammation of the right lobe of the liver. In Cases II and III this symptom was absent; at all events, the patients made no complaint of it; whereas in Case I, where the suppuration involved the lobulus Spigelii, and the right lobe was unaffected, constant pain was experienced between the shoulders.

In Case II, the malignant disease had probably been making progress for some time before the man's admission. His sallow and cachectic look led us to diagnose *this*. On the 14th April, exactly a month after he had been under treatment, the pain was more localised, and accompanied by vomiting. It is not unlikely that some peritoneal inflammation was caused at this period by the cancerous growths, and that the inflammation and suppuration of the adjacent hepatic tissue were secondary to this peritonitis.

Original Communications.

LECTURE ON THE SOUNDS OF THE HEART,

DELIVERED IN THE LIVERPOOL ROYAL INFIRMARY SCHOOL OF MEDICINE.

By A. T. H. Waters, Esq., Lecturer on Anatomy and Physiology, etc.

Gentlemen,—In the present lecture, I propose to consider the causes of the sounds of the heart. The subject is one on which the minds of physiologists seem to be by no means settled; and a careful and candid inquiry into the different causes which have been, or still are, considered to be equal to the production of the phenomena, will, I venture to hope, not be unattended with benefit.

If you place your ear over the præcordial region, you will hear two sounds: one heard most distinctly towards the apex of the heart, dull and somewhat prolonged; the other heard best at the base of the heart, less prolonged and sharper in its character. These sounds are called the *first* and *second* sounds of the heart. The first is synchronous with the contraction of the ventricles and the propulsion of the blood into the aorta and pulmonary artery, and slightly precedes the pulse at the wrist. The second immediately succeeds the first, and is synchronous with the relaxation of the ventricles, and the attempt of the blood to regurgitate from the aorta and pulmonary artery.

Experimental investigation into the causes of the sounds of the heart is of recent date; and at this we shall not be surprised, when we learn that, up to the close of the last century, so little was known even of diseases of the organ, that the only two recognised were polypus and palpitation—the latter being considered a nervous affection.

The first object which I propose to myself is, to trace down from the earliest periods to the present time the history of the different theories which relate to the subject of the lecture.

The theory that muscular contraction, or, in other words, the successive shortening of the muscular fibres, produced the sounds, appears to be the oldest, and is the one advocated by Galen, Senac, Haller, Bichat, and Corvisart. The immortal Laennec embraced the same opinion as his predecessors. He attributed the sounds to the alternate contraction of the auricles and ventricles. "One of the sounds," he says, "is clear and rapid, and corresponds with the systole of the auricles; the other is more dull and prolonged, and corresponds to the systole of the ventricles."

This theory of Laennec remained unquestioned for some years, until it was shown—which would have been at once apparent, if the rhythm of the heart had been better understood—that the contraction of the auricles precedes that of the ventricles; whereas the sound that Laennec attributed to their action—viz., what we call the second sound—immediately succeeds it.

The physiologist who first disproved Laennec's view was Mr. Turner; and he endeavoured to account for the second sound, by supposing that it was produced by the heart falling back on the pericardium, after the ventricular systole. This is disproved by the fact, that the sounds are heard when the pericardium is removed; and further, that the pericardium, in a state of health, is always in close contact with the heart, so that the latter cannot fall back upon it.

Another theory, for which we are indebted to Dr. Corrigan, was, that the impulse and first sound were produced by the rush of blood into the ventricles during the auricular contraction; and the second, by the ventricular systole, which he considered instantaneous. These conclusions are incorrect, because the first sound takes place not synchronously with, but subsequently to, the auricular contraction; and with regard to the second sound, the conclusions are untrue, because the ventricular systole, and the pulsation in the arteries near the heart, do not coincide with, but precede, the second sound.

M. Pigeaux expressed an opinion similar to that of Dr. Corrigan, with regard to the first sound; and of the second, he says: "it is caused by the collision of the blood against the walls of the aorta and pulmonary artery." This is disproved by the fact, that the pulsation in these vessels precedes the sound.

The celebrated physiologist Magendie has given us the following view of these sounds. "The first sound," he says, "is produced by the apex of the heart coming in contact with the ribs during systole; and the second by its anterior surface coming in contact with the sternum during diastole." These conclusions are evidently incorrect, inasmuch as both sounds are distinctly heard after the sternum and ribs are removed.

M. Marc d'Espine says: "The first sound is due to the pure and simple effect of the ventricular systole." This is a recognition of *bruit musculaire*, to the exclusion of all other sources of sound. "The second sound," he says, "is the pure and simple effect of the ventricular diastole." This recognises the power of the muscle to produce sound in relaxation, and excludes the valvular sound.

We are indebted to Mr. H. Carlile for another theory, viz.: that the first sound is produced by the rush of blood into the arteries during the ventricular systole. This is a view held at the present day, and to which I shall have again to refer. He says that the second sound is due to the closure of the semilunar valves.

In the year 1832, Dr. Billing published a view of the causes of these sounds, which he had previously taught to his class, and which has been lately proved to be correct by actual demonstration; viz., that they are both produced by the forcible closure of the two sets of valves; the first, by the closure of the mitral and tricuspid valves; the second, by that of the semilunar valves.

This view was afterwards adopted by Rouanet and Mr. Bryan, and with some slight modification by Bouillaud.

Dr. C. J. B. Williams, who performed many experiments with reference to these sounds, has adopted the muscular theory to account chiefly for the first sound. To this, I shall again have to refer.

In the year 1830, Dr. Hope performed a number of very interesting experiments, with the view of proving what were

the causes of these sounds. The conclusions he came to were, "that the first sound was caused by the systole of the ventricles; and the second by their diastole." From subsequent experiments, he had reason to alter his opinion with reference to the cause of the second sound; and in 1835, he published the following view. "That the first sound was compound; viz., consisting—1st, of valvular sound; 2nd, of the sound of extension, 'a loud smart sound, produced by the abstract act of sudden jerking extension of the braced muscular walls'; 3rd, a prolongation and possibly augmentation by *bruit musculaire*. That the second sound was produced by the sudden expansion of the semilunar valves, resulting from the recoil upon them of the column of blood in the aorta and pulmonary artery."

If you consult the different works on physiology and medicine, where reference is made to these sounds, you will find that the views of authors of the present day are by no means settled on the subject. The difficulty seems to be with the first sound; for the experiments of Hope proved that which Billing propounded with reference to the second sound—viz., that it was due to the closure of the two sets of semilunar valves.

Let us next examine into the events which are taking place in the heart at the time when these sounds are produced, and then ascertain the opinions most generally entertained with reference to their causes at the present day.

The events which correspond with the first sound are—first stage of dilatation of auricles; contraction of ventricles; closure of auriculo-ventricular valves; opening of ventriculo-arterial valves; propulsion of blood against auriculo-ventricular valves, and through the orifices of aorta and pulmonary artery; impulse of heart.

The events corresponding with the second sound are—relaxation of ventricles; dilatation of auricles; backward flow of blood in aorta and pulmonary artery towards ventricles; closure of semilunar valves.

From the time of Laennec up to the present day, as many as twenty-nine theories have been proposed to account for these sounds. To some of them I have alluded; and I now proceed to give you the principal ones held at the present day.

We have, as alleged causes, 1. Impulse; 2. *Bruit musculaire;* 3. Tension of the auriculo-ventricular valves; 4. The rush of blood throughthe narrowed orifices of the great arterial trunks; 5. The collision of the particles of blood with one another, and with the parietes of the heart; and, lastly, the pressing back of the semilunar valves. Such are the theories with regard to the first sound. With regard to the second, scarcely any difference of opinion exists; it is acknowledged to be due to the closure of the semilunar valves.

Let us now examine the various causes above alluded to. That impulse has nothing whatever to do with the first sound, is most satisfactorily proved by the following experiment; viz., that on removal of the anterior part of the thorax, and on placing the stethoscope on the surface of the heart itself, the sound is in no way whatever diminished in intensity, but if anything more distinctly heard than before. This observation was made by Dr. Hope, and has been confirmed by all subsequent experimenters. Independently of this demonstrative proof, it is difficult to imagine how the impulse could produce a sound, inasmuch as the heart does not strike the chest, always being in contact (in a state of health) with its anterior wall; but simply makes it bulge, as the ventricles during contraction assume a globular form.

I pass over for the present the second and third causes assigned, and come to the fourth, viz., the rush of blood through the narrowed orifices of the great arterial trunks. This view is assumed from the fact that sound is produced by pumping fluid through tubes out of the body, and further that whenever the calibre of a vessel in the body through which blood is circulating is diminished, a sound is produced. That such an analogy as is here assumed, exists between the forcible and sudden pumping of fluid through an inert tube out of the body, and the action of the heart in impelling its blood into the elastic and yielding vessels situated at its base, I cannot admit; nor is the condition of these vessels at their origin at all similar to that of a compressed artery. There is no natural impediment to the onward flow of the blood, the parts leading to the vessels are perfectly smooth, and the vessels themselves are of so elastic and yielding a material, that they dilate in every direction when they receive the blood, as can be readily felt if they are grasped during systolic action of the ventricles. If the arterial orifices were compressed during the time the blood was passing through them, so that their shape would be altered and their calibre diminished, then the same result would take place as in the arteries elsewhere; but no such compression occurs, nor is there any augmentation of sound, over the base of the heart as there ought to be if this view were correct; on the contrary, when the heart is exposed, and a stethoscope applied to it, the sound is found to be most intense over the situation of the auriculo-ventricular valves.

That the fifth cause assigned, viz., the collision of the particles of blood amongst themselves and with the parietes of the heart, a view advocated by some physiologists, can be an element in the production of the first sound, I can scarcely imagine. It appears to me that if such were the case, it would produce an almost continuous murmur, for there must be collision equally during diastole of the ventricles, and especially when the contents of the auricles are propelled into them, as during systole, and sound ought to be produced during the one act, as well as during the other; but such is not the case; no audible sound is produced by the propulsion of the blood into the ventricles, and it is but fair to infer, that none is produced by its propulsion from them.

I pass to the consideration of the muscular sound, the *bruit musculaire*. It has always been the opinion of physiologists that the contraction of the muscular walls of the ventricles, was either the sole cause, or an element in the production, of the first sound. Dr. Hope adopted the latter view, and it is generally entertained at the present day.

If you place your ear or a stethoscope over a muscle during its contraction, you will hear a sound, but in its character it differs entirely from the first sound of the heart. Perhaps you cannot have a better example of the sound produced by muscular contraction, than that afforded by the action of the masseter muscle. If, when your head is resting on a pillow, you forcibly contract your masseter muscle, you will hear a rumbling noise—a noise which continues during the contracted condition of the muscle, and is also heard during its relaxation. The exact cause of this sound I am unable to tell you. It may be due to the change in shape of the muscular fibres—it may be due to the alteration of the circulation—or to some other cause; but the more you examine it, and all other muscular sounds, the more will you be convinced, that they bear no resemblance to the first sound of the heart.

Before alluding to the experiment which some of you have witnessed, and which proves beyond all doubt that the contraction of the ventricles produces no sound, I think it right to bring under your notice, other arguments which tend to prove the point. If the ventricles during contraction produced sound, it is but fair to infer that the auricles would do so. We find, however, that such is not the case. If it be said that the muscular substance of which the walls of these cavities are composed, is so slight, that no audible sound could be expected from its contraction, I maintain that such an argument is unsound, and I would ask, what is the exact amount of muscular fibre necessary to produce an audible sound? Are not the auricles of a full-grown man as large, and do they not contain as much muscular fibre, as the ventricles of a *fœtus in utero*?—and shall we admit that the contraction of the latter will produce the clear audible click, with which many of you are familiar, and deny that the former can have any such effect at all? Again, presuming that the auricular fibres in man are not sufficient in number to produce a sound, are there not animals which possess auricles larger than the ventricles of some adult men?—but yet, if you listen to the sounds produced by their hearts (as, for instance, the ox or the horse), you find that they are the same in number and character as those of the human being—during the auricular contraction no sound is heard. These facts seem to me conclusive against the muscular theory, and to render it perfectly untenable.

We are not, however, without positive evidence of the truth of the assertion, that the ventricular contraction produces no sound. To Dr. Halford, who has lately brought the subject under the notice of the profession in London, and who has performed his experiments in this theatre, we are indebted for the beautiful experiment which some of you have witnessed.

The experiment consists in depriving an animal (dog or donkey) of sensation by means of chloroform; and, whilst artificial respiration is kept up, the anterior part of the thorax and the pericardium are removed and the heart exposed; on listening to the heart, the two sounds are heard. For the remaining part of the experiment, I quote Dr. Halford's own words.

"The superior and inferior venæ cavæ, and the pulmonary veins, were now compressed between the fingers, and the heart continuing its action, a stethoscope was again applied, and neither first nor second sound was heard. After a short space of time, the veins were allowed to pour their contents into

both sides of the heart, and both sounds were instantly reproduced. The veins were again compressed, and all sound extinguished, notwithstanding that the heart acted vigorously. Blood was again let in, and both sounds restored. All that is claimed for the above experiment, is its exemption from any rude interference with the mechanism of the heart's action. The cavities of the heart are untouched; there is no finger thrust into the auricle or ventricle; no hooking back of valves: in fact, not one source of sound substituted for another. Both sounds are destroyed and reproduced by the same means; the strongest argument for their both depending on the same cause, which is simply the backened current of blood, first against the auriculo-ventricular, and second against the ventriculo-arterial valves."

Now what is the value of this experiment? It proves unquestionably that there is no such thing as *bruit musculaire* in connection with the systole of the ventricles, and it disproves all the theories founded on a contrary supposition; it proves, moreover, that whenever the blood is allowed to play upon the valves, the sounds are produced.

Before Dr. Halford performed his experiments, in the original of which I assisted him, a very ingenious experiment had been performed by Mr. Brakyn, which consisted in propelling, by means of bladders and tubes connected with the left side of the heart, air through the cavities, so as to represent the flow of the blood. His experiment proves that when the auriculo-ventricular and the ventriculo-arterial valves are thrown into a state of tension, by air acting on them in the same way as the blood, sound is produced. This experiment I have frequently repeated—the original apparatus used by Mr. Brakyn being still in my possession—and the sounds resemble in every respect, considering the nature of the fluid in which they are produced, those of the living heart.

These two experiments seem to me to settle entirely the question which has so long agitated the minds of physiologisis. Mr. Brakyn's proves that the tension of the valves is equal to the production of the sounds, and Dr. Halford's that muscular contraction has nothing to do with them.

The pressure of the semilunar valves against the sides of the great vessels is said to assist in the production of the first sound. This requires but slight consideration. Against what are they pressed? Against the *yielding* walls of the vessels, and can there be any element of sound in such an occurrence? I think not.

I think I have now proved that all the phenomena synchronous with the first sound, except the closure of the valves, are unequal to its production, or even to assist in it; and also, that such closure is equal to produce the effect. It is then to *the tension of the mitral and tricuspid valves, produced by the blood being forcibly propelled against them*, that this sound is due.

With regard to the second sound, there is but little difference of opinion; the experiments of Hope proved that it was solely due to the semilunar valves. When the arteries recoil after being distended by the ventricular systole, the blood in them is forced back towards the ventricles, the semilunar valves then come into play, they are stretched across the vessels, and suddenly made tense, and then sound is elicited.

If you listen to the sounds carefully, you will find that they do not differ in *kind*, but only in *degree*. The auriculo-ventricular valves are large, thick, and strong; consequently their vibrations are slow, and the sound they produce prolonged. On the other hand, the ventriculo-arterial valves are small, thin, and comparatively weak, and their vibrations are rapid, and the sound they produce short. Both sounds may be illustrated by making tense two pieces of membrane of different size and thickness.

If the theories I have mentioned require any further confirmation, they receive it from the sounds which result when there is disease of the heart. If you hear a murmur, you know there is something wrong with the valves, you do not think of the muscular walls; you know that there is some deposit in connexion with the valves, which either from its roughness causes a sound as the blood passes over it, or else prevents the valves properly closing, and thus allows of regurgitation.

Moreover, consider the alteration in the sounds, which is the result of a change in the muscular walls. When the ventricles are hypertrophied, the first sound is less distinct, and of a muffled character; it has to pass through the thickened muscle, and necessarily comes less sharply to the ear. Again, when there is dilatation of the ventricles and thinning of their walls, the sound is clear and sharp; it has to pass through a smaller space and is less altered in its character. If the sound were due to muscular contraction, surely, in obedience to the law of physics, that if you increase the cause you increase also the effect, the sound of a hypertrophied heart would be louder and more distinct than that of one in which the muscular fibres are diminished; but the contrary obtains, and this fact affords an additional proof of the valvular theory.

REMARKS ON DR. HALFORD'S EXPERIMENTS CONCERNING THE SOUNDS OF THE HEART.

By W. O. Markham, M.D., Assistant Physician to St. Mary's Hospital, London.

Dr. J. B. Halford has lately performed a series of experiments at different medical schools in London, for the purpose of demonstrating, amongst other things, that the opinions long ago laid down by Dr. Billing, respecting the nature and causes of the sounds of the heart, are correct; viz., that both the sounds are entirely and alone produced by the valves of the heart.

It would appear from remarks, which have been made in some of the medical periodicals, upon those experiments, that they have been by many persons received as positively demonstrative of the opinions above mentioned. As I happened to be a witness of the experiments which were made at St. Mary's Hospital, and as I could not convince myself of the correctness of the above conclusion from what I then and there observed, and as I think them quite untenable on other grounds, perhaps I may be permitted to state, in a few words, my reasons for being so unfortunate as to differ from many others, who have assisted at these vivisections.

The position assumed by Dr. Halford is this:—When no fluid—liquid or gaseous—passes through the cavities of the heart, the valves are not called into action, and no sounds are produced. Hence, the sounds of the heart depend upon the action of the valves. In order to demonstrate this position, Dr. Halford rapidly opens the thorax of a dog under the influence of chloroform, and by the aid of a bellows inserted into the trachea keeps the animal alive by sustaining the respiratory functions. He then skilfully cuts off all the sources through which blood, venous or arterial, can find its way into the heart's cavities. When this has been effectually accomplished, Dr. Halford finds that the sounds of the heart are no longer audible.

In the two cases in which I saw Dr. Halford operate, I could not admit the correctness of his views; and for the reason, that I still heard two sounds associated with the movements of the heart, although he assured me that all sounds were at the moment inaudible.

Certainly, the sounds I heard differed vastly from the healthy and natural sounds of the heart; they were weak, dull, and muffled, resembling rather the obscure flutterings of a heart rapidly and feebly beating in the last agony; but, nevertheless, of such a character, there they were to be heard, at least by the evidence of my sense.

I am well convinced, from the very nature of the experiment, and from the difficulty of rightly manipulating with the stethoscope under such circumstances, that the sounds might be readily overlooked; and I can quite understand that many persons might not be lucky enough to catch them. However this may be, it is clear that the negative evidence of numbers cannot destroy the positive evidence of one's own senses; and therefore I cannot admit, on this ground, that Dr. Halford's experiments prove the correctness of the position which he takes up.

Admitting, however, the incorrectness of my own observation, and assuming the correctness of Dr. Halford's—viz., that in the cases before us no heart's sounds were audible—I really think some objection may be very fairly taken to the sweeping views thence deduced by him. Is it right, one might ask, to assume that in an animal thus mutilated, and subjected to sudden and violent shocks of the nervous, arterial, and respiratory systems, *all* the elements which may possibly conduce to the formation of the heart's sounds can be left in undisturbed action? Surely, all the other possible causes which may form, or assist in forming, the heart's sounds, as they are heard during healthy life and under a natural condition of existence, must be eliminated from the calculation, before Dr. Halford can assume, as he does, that to the motion of the valves, and that to it alone must be ascribed the cause of the sounds in question. What becomes of the impulse of the heart against the thoracic walls? How is the muscular bruit to be got rid of? How is the rush of blood through the heart's orifices and

over the roughened surfaces of its cavities to be explained away upon such an assumption as this? Dr. Halford may ignore the muscular bruit, and declare the impulse of the heart's apex against the thoracic walls a creature of the imagination; but I am sure that he will not persuade many stethoscopic observers to agree with him herein.

Dr. Halford's, as well as Dr. Billing's views, are surely in this matter too exclusive. No one can doubt that the valves have a most important share in the formation of the heart's sounds; but certainly Dr. Halford's experiments do not prove that no other causes may contribute to their formation; most assuredly, they do not disprove the possibility of the existence of a sound from the impulse, of a sound from the muscular contractions of the heart, and of a sound from the rush of blood through the heart.

It must be admitted, that we have gained a great deal of knowledge concerning the sounds of the heart from observation of their alterations in diseased conditions of the organ; and we may, I think, safely assume from a fair consideration of the same class of facts, that much obscurity still involves the question of the nature and origin of the sounds. The pathologist at the bedside of the patient is continually meeting with facts which seem repugnant to his received theories, and even to what seem to be demonstrated facts. I will here mention one, because it seems to show in a very striking manner the great difficulty which hangs over the subject.

It is generally admitted by observers, that the second sound depends altogether upon the falling together, during the heart's diastole, of the semilunar valves of the aorta and pulmonary artery; this is a fact, which would appear to be placed almost beyond the reach of discussion, being readily demonstrable and decisively so. Notwithstanding, however, the demonstrations of physiological experiments, pathology steps in to throw doubts upon their correctness. How comes it, for instance, that in certain cases of mitral valvular disease, very frequently to be met with, no second sound at all is heard at the apex and about the left side of the heart? A loud *bruit* is heard accompanying the first sound at the apex, but no second sound or *bruit* is audible there. How comes this, when at the same time the second sound is heard loud and clear over the situation of the aortic valves; and loudly intensified, it may be, over the base of the pulmonary artery? Why is the second sound, as usually heard over the apex of the heart, not heard now, if its cause is always to be sought in the semilunar valves? What should prevent the sound being conveyed downwards to the apex during the heart's diastole in this case, as well as during health? I certainly cannot see.

How, again, are we to account, on Dr. Halford's theory, for the reduplicated beat so often heard over the ventricles during their contraction? How also are we to explain the fact, of the second sound being occasionally loud and clear over the ventricles, when at the same time it is very weak and almost inaudible at the base of the heart, if we are in every case to attribute its formation entirely to the closure of the semilunar valves?

Pathological and physiological considerations thus justify us in believing that the causes of the heart's sounds are complex, not simple; that they are produced by no single act, but that several acts are associated in their formation; and that in fact, there is much truth in the assertion made by Skoda, viz., that the ventricles, the aorta, and the pulmonary artery, severally contribute in the production both of the first and of the second sounds of the heart.

CLINICAL OBSERVATIONS ON THE SPECIAL APPLICATION OF LIQUOR PEPSINIÆ IN CERTAIN DISEASES.

By DAVID NELSON, M.D.Edin., formerly Physician to the Queen's Hospital, and Professor of Clinical Medicine, Birmingham.

SINCE my last remarks upon this subject, I am glad to observe, from the discussions called forth, and the attention paid to the preparation of the medicine by chemists, that it is now taking that hold upon the professional mind which its value deserves. In papers previously published, I have adverted to the successful treatment of certain cases of diabetes several years ago by means of this remedy, and also to its general value as a natural solvent of the food within debilitated stomachs. In regard to diabetes, I have now about a dozen patients in whom there has occurred a complete arrest of that usually deadly malady; but I do not intend at present to treat of that particular disease, or to enter upon any theoretical discussion thereon, but to lay before my brethren of the BRITISH MEDICAL ASSOCIATION some evidences of the special value of this agent in chronic complaints, and in those forms of disease which are viewed as malignant, or which are of so obstinately destructive a kind that their ultimate results are quite as bad. I would wish it to be understood at the outset, that, in citing such evidence, I have no intention of making perfect reports of the cases, in their rise, progress, treatment, and termination; but mean simply to mention such salient features of the ailments as may tend to illustrate the beneficial action of pepsine in arresting or retarding their advance.

I would also premise, that the form of the agent employed has been that of the *liquor pepsiniæ præparatus*, or *medicinal rennet*, as made in Birmingham, the ordinary dose of which is one fluid drachm. I shall also endeavour to classify the cases so as to exhibit its effects methodically, beginning with those of a milder character, and proceeding to others of a more formidable or fatal tendency. With this view, I shall illustrate its effects, *seriatim*, upon patients labouring under simple chronic dyspepsia; upon others labouring under dyspepsia associated with affections of the heart, lungs, uterus, or brain; upon some with abdominal tumours; upon some with ulceration of the stomach; upon others with malignant disease of the liver or stomach; upon others of a tuberculous habit, but especially young children with tabes mesenterica; upon one with diabetes, recently treated with success, but not included in any former report; and on another with albuminuria.

A. SIMPLE CHRONIC DYSPEPSIA.

CASE I. Mr. G. G., of Staffordshire, was a farmer, advanced in life, and had suffered for many years from indigestion; otherwise, as regarded all his vital functions, he seemed perfectly sound. His circulation and respiration were good, his complexion was ruddy, and he was strong and active in all his limbs; but he never ate a meal, however simple, without suffering from distension, severe pain, protracted eructations of flatus, and sometimes vomiting. I had formerly prescribed for him vegetable bitters, with alkalies, bismuth, and hydrocyanic acid, with only limited and temporary benefit; but, on taking the three latter medicines with the liquor pepsiniæ, an immediate and continued favourable change was reported. Afterwards, he took the peptic liquor only, with a little alkali; and, as he expressed it, he "ceased to know he had a stomach."

CASE II. Mrs. S. C., of Walsall, corpulent and unwieldy, 45 years of age, had been troubled with indigestion for years past. She never had pain; but her bowels were irregular in their action, and she had constant eructations day and night. She was muscular, but her muscle was imbedded in fat; and the abdomen was immensely large, pendulous, and tympanitic. She had used ordinary remedies without much change, and had never restricted her diet, as she had a good inclination for food, and regularly ate four full meals a day. On using the liquor pepsiniæ with an alkali, and taking two aloes and assafœtida pills at night, instead of supper, her uncomfortable feelings left her, and, with continuing the occasional use of the medicine, and the avoidance of pastry and beer, and the adoption of a meat and bread and wine diet, she has ceased to complain.

CASE III. Mrs. A. C., of Staffordshire, was a lady of tall and firm frame. She consulted me for an obstinate attack of rheumatism, which had specially seized on the tendons of the neck, so as to induce stiffness and great anguish on moving the head or arms. On inquiring into her general health (the state of the stomach included), she said that, eat what she might, for some time past, the food not only lay heavy, but turned sour, and produced at least once a day, and very often in the night, a violent and protracted attack of spasm. She added, that hot water and mustard plasters, with brandy, ether, opium, and chloroform, had all been used, with only transient benefit; and therefore concluded that she must put up with it for life. A soothing and absorbent liniment was prescribed; and she took first fifteen grains of bicarbonate of soda, ten grains of bismuth, fifteen drops of wine of colchicum, one drachm of liquor of pepsine, and twelve drops of solution of muriate of morphia in water, three times a day, after meals. Her rheumatism subsided entirely in from two to three weeks, all the fibrinous swelling having left her neck; and, from the time that she took the first dose of her mixture, she had no spasm of the stomach. She sent her cook, a few weeks ago, to me, requesting the favour of my attending to her for sundry anomalous symptoms, which disappeared after the expulsion of a large round worm eight inches long; and, on my asking how her mistress was, she

said, "Quite well, sir: we are never now roused up at night for hot water and so on."

B. DYSPEPSIA ASSOCIATED WITH AFFECTIONS OF THE HEART, LUNGS, UTERUS, OR BRAIN.

Case I. Mrs. F., a lady of 36, first consulted me at the acme of an attack of hepatic congestion, with its consequences. The head was hot, and ached severely; the appetite was gone; the tongue was covered with a thick fur; the bowels were constipated; and the urine was thick with lithates. She was more than usually short of breath, owing to an asthmatic tightness. There were tenderness and fulness in the region of the liver; the heart paused from time to time; there was a rough murmur in the course of the pulmonary artery; and the legs were swelled. Although some fear of tubercle had been expressed both here and in London, the fulness of the abdomen, along with the breadth and shortness of the chest, and other signs, dispelled that idea from my mind; and I rather inferred that all the train of symptoms were primarily traceable to flaccidity of the heart and obstruction at the pulmonary vessels. The first treatment consisted in the application of leeches to the right side, and the exhibition of purgatives, along with antispasmodic diaphoretics and ammonia. Under these appliances, the gastro-hepatic symptoms disappeared, and the respiration became easier: still the pulse was faint and intermittent, and the *bruit* continued; and she could not move into the cold, or go up stairs, or dress or undress, or partake of such food and drinks as her debility required, without bringing on heartburn, with asthmatic wheezing, faintness, and violent palpitation. Quinine and steel were now ordered with the food, and alkalised pepsine after it, as well as powerful antispasmodic stimulants, as an extra support when required by the urgency of the distress. After a short period, the pulse became more regular, and the asthma left; she was able to drive and walk about with ease; the legs regained their natural tapering form; she could eat beef, and take a glass or two of port wine at dinner, without heartburn or palpitation; the action of the heart became steady and strong; and, in short, she was well, except for the medical perception of what existed in the pulmonary artery, threatening, of course, a return of all the old evils, whenever the directions as to diet, etc., might be neglected. Her brother had been found dead in bed, and the *post mortem* examination had revealed some heart disorder in his case.

Case II. Mrs. S. D., a married woman, 40 years old, had been afflicted with asthma for many years, always much aggravated under attacks of cold, to which she was exposed in her occupation of shopkeeper. It was associated, as usual, with habitual dyspepsia. Her diet was irregular, and often improperly cooked, frequently consisting of hashes, for the sake of convenience. She consulted me for an acute attack of bronchitis. There was extreme dulness on percussion over the whole chest; the respiration was labouring and very sibilant; and the secretion of tenacious mucus, which could not be cast off except by vomiting, was immense. There was dull pain in the region of the liver, and great tenderness at the epigastrium, the slightest touch of which induced cough and increase of the dyspnœa. She was first treated with emetics and purgatives, conjoined with soothing and antispasmodic diaphoretics; and, when the chest and stomach, etc., had thus been relieved of their loads, she commenced the pepsine after food, which of course was ordered to be simple. Since then she has been very much less troubled with her asthma, which was wont to be a regular nightly visitor, and a torment for weeks together when she caught cold, or when anything disagreed with the stomach. When an attack threatens, an emetic, with a short course of liquor pepsiniæ and diaphoretics, now cut it short.

Case III. Mr. G. D., a firework maker, and exposed to fumes of sulphur, etc., consulted me in an extremity of distress, arising from extensive bronchitis and asthma. He was considered as going fast in phthisis; but, although he was thin and pale to an extreme degree, with a shrunken chest, dyspnœa, and hæmoptysis, I was not disposed to adopt that opinion, on account of the slowness of the pulse and the duration of the disease. His respirations, under the slightest exertion, were then 60 in the minute. Percussion over the chest proved general dulness; and all pulmonary sounds, whether sibilant, snoring, chirping, creaking, crackling, or gurgling, could be heard in one or other part of the chest, the respiration being also highly cervical and abdominal. I considered that repeated attacks of bronchitis had thickened the mucous membrane universally, caused the proper tissue of the lungs to shrink, induced adhesions, and brought on both organic and spasmodic asthma, with consequent retention of morbid mucus. There was neither appetite for much food, nor power of digesting the little taken. He described himself as alternately ready to faint and sink from a sense of suffocation. He was ordered first an emetic, which was followed up by antispasmodic and stimulant diaphoretics; and the pepsine was administered after meals. The chest was also blistered. Under a course of this treatment, he gradually recovered to the extent of reducing his respirations to 24; but, from the first, he said that the medicine after food had done him much good by relieving him of those dreadful attacks of asthmatic dyspnœa which had formerly invariably ensued upon meals, and which made him afraid to eat even while he felt inclined. He continues pale, thin, and weakly; but the chest-sounds are merely universally rough, and he can work and walk about with tolerable ease. He takes the pepsine liquor with his food, and lobelia at bedtime when required.

Case IV. Miss J. S., of Worcestershire, 48 years of age, unmarried, and exposed to no privation, having a cottage of her own and a small independency, consulted me in regard to habitual indigestion of some two or three years standing. It was accompanied with violent attacks of pain, sometimes in her stomach, and sometimes in the lower part of her abdomen. The latter spasms radiated from a point just behind and under the pubes, and extended to the sacrum, down the thighs, and up to the umbilicus. She reported that she had previously seen consulting practitioners, who, after treatment, had usually come to the final conclusion that the symptoms were hysterical; but, hysterical or not, they had not left her. She was ruddy in complexion, and not excitable, but of good sense and calm demeanour; and, finding nothing organic to lay hold of at first, I suspected a worm, and prescribed accordingly. No worm appeared, and the symptoms continued as before. A digital examination of the uterus was then made, when the os was found to be exceedingly tender, and somewhat puffy and flaccid. She said that, for some time past, menstruation had been very painful, and that griping pains had continued in that quarter even after the flux had stopped. She was ordered a mixture containing liquor pepsiniæ and bismuth, with muriate of morphia for her stomach and other pains; and leeches to the vulva. This treatment gave relief to the digestion, and for a time abated the uterine pains; but, as they recurred in force as before, a specular examination was made, which resulted in the detection of a small protruding polypus, brilliant in colour, narrow, and about a half-inch in length. A skilful surgeon, who afterwards examined, stated his inability to remove it, on account of the rigidity of the parts, the smallness and softness of the growth, and the depth and narrowness of the passage. In a short time, however, it was freely treated with nitrate of silver, on sundry occasions; and, although there seemed no change at first, it ultimately disappeared, with a complete relief to all the local pains. The stomach does continue weak, and she lately remarked, "I feel so well that it almost seems foolish to be taking medicine; but as soon as I stop this for the stomach, I begin to have pain and flatulence as before."

Case V. Mrs. J. M., a married woman, aged about 35, and without family, consulted me for a swelling and severe pains in her abdomen. She said that all her food disagreed with her, and so added to the ordinary distension of the belly that the agony was almost insupportable, and she could scarcely breathe. She had previously taken pills, which relieved the pain by making her stupid and drowsy; but they bound her bowels; and, when they did not produce sleep at night, she was haunted with horrible visions. On examination, it was clearly a case of immense ovarian disease, the tumour occupying nearly the entire abdomen. The pressure of this gave rise to the disturbed digestion, flatus accumulated, and so came the paroxysms of pain and distension. She was ordered to evacuate the bowels daily by means of a warm decoction of poppy in full quantity. She took a stomachic mixture containing the liquor pepsiniæ; and, instead of any opium, had muriate of morphia at night when she did not sleep. This led to a subsidence of the paroxysmal attacks. The solid tumour, of course, remains as before; but she is able to work at a lathe, and feels nothing beyond the sense of weight and oppression, and an occasional lancinating pain through the tumour.

Cases VI and VII. M. H. and J. H., brother and sister, were brought to me from Walsall, both labouring under epileptic fits of very frequent occurrence. The girl was 16 years of age, of ruddy dark complexion, short and round in her make, and her upper and lower extremities fully developed, and remarkably solid and firm. The lad, aged 12, was quite of the same robust organisation. The heart and lungs of both were sound; the

intellects clear; the abdominal organs vigorous; and, in fact, the only thing the mother could see was, that they were of a very "consumptive nature" as regarded food, which, however, seemed to make them sleep heavily, yet restlessly; and they also had the fits. They both confess to always feeling "blown out" after their meals. In case of any worms, they were first ordered a succession of calomel and aloes purges, followed up by the exhibition of male fern. This latter agent expelled no worm; but, on the other hand, fits occurred shortly after its being taken. On the next visit, purgative pills were prescribed at night, with quinine, zinc, and silver, thrice a day; and the liquor pepsiniæ after each meal. Of course these meals were ordered to be reduced and made simple. From that time, through a period of between two and three months, no "blowing out", nor night restlessness, nor epileptic fit, took place. They continue well, and are instances of epileptic convulsions occurring in robust bodies from over-charging of the stomach, in which cases all that is wanted is restoration of digestive power.

CASE VIII. Mr. T. J., aged about 70, and who had once before had an attack of apoplexy, had ventured upon an excursion into the country, with two or three of his sons. He was in good health and spirits in the morning, and, before departure, partook of a hearty breakfast of meat and eggs. He enjoyed himself, and felt well appetised after reaching the place of destination by horse-carriage, and dined almost voraciously, and without much chewing, as he was defective in his teeth. He also enjoyed the return home, felt again hungry, and had some roast veal. Before going to bed, he began to feel sensible of the weight at his stomach; but no alarm arose. In the course of the night, his wife remarked that his snoring was remarkably loud and deep, and, on endeavouring to rouse him, she could not succeed. Her experience of the former attack excited her fears. The family were summoned, and first a surgeon, and afterwards myself, were requested to attend. The poor old gentleman was quite insensible, and his eyes fixed. The pulse was slow and full, and the only thing that excited a groan was pressure of the epigastrium. The first appearance and the history of the case reminded me much of that of the late Duke of Wellington—stupefaction supervening upon a heavy meal in an aged subject. Leeches were ordered to his head, and powerful injections by the bowels; while it struck me that the liquor pepsiniæ might aid the favourable issue by dissolving any crude food, an emetic not being justifiable, under the impression of cerebral congestion, in so old a man. The leech-bites bled freely; and, while the injections were acting, vomiting also spontaneously occurred, and he ejected from the stomach an immense quantity of solid material, including vegetable fibre, and especially masses of hard gristle belonging to the veal. He was now able for a few times to be got out of bed, but afterwards relapsed into stupor, from which he only partially emerged to move his right hand over his right eye, and groan deeply; while his left arm was paralysed, and his mouth drawn to one side. He was now blistered over the forehead, put under a course of mercury and iodide of potassium, and went on with liquor pepsiniæ and spoon-food. His first exhibition of improved consciousness consisted in a desperate warfare of the right hand against the blister; and his first essay in the use of the tongue was in answering "yes" to every possible question. He next observed objects fixedly, but without recognising them. One of his earliest blundering attempts resulted in his calling his wife "the old clock"; and, on another occasion, after attentively studying the face of my watch, he first said "twelve"; then, carrying his finger on to the 1, and 2, and so midway to the 3, he exclaimed, "Twopence halfpenny to 12 o'clock;" and seemed exhausted with the effort. From this condition, however, he gradually and slowly recovered, with the loss of vision in one eye, and some impairment of memory. On my meeting him lately, he lifted his hat and repeated my name. He can walk the streets by himself, and, with care, remains in good health. Though liquor pepsiniæ can have no power over the essential nature of apoplexy, yet I believe life may often be saved by its solution of crude food, and maintenance of a sound digestion, while more active means are being carried out.

[*To be continued.*]

MEDICAL SOCIETY OF LONDON. On this (Saturday) evening, a paper will be read by H. Hancock, Esq., "On a Case of Excision of the Floor of the Acetabulum and Head of the Thigh-bone, where Carious Perforation existed with Pelvic Abscess."

Association Intelligence.

BRANCH MEETINGS TO BE HELD.

NAME OF BRANCH.	PLACE OF MEETING.	DATE.
BATH AND BRISTOL. [Ordinary Meeting.]	White Lion, Bristol.	Thurs., Feb. 19, 8 P.M.
METROP. COUNTIES. [Adjourned Meeting.]	37, Soho Square, London.	Tues., March 3, 4 P.M.

ADMISSION OF NEW MEMBERS.

THE Laws of the BRITISH MEDICAL ASSOCIATION, with regard to the admission of new members, are the following:—

Any qualified medical practitioner, not disqualified by any bye-law, who shall be recommended as eligible by any three members, shall be admitted a member at any time by the Committee of Council, or by the Council of any Branch.

The subscription to the Association shall be One Guinea annually; and each member on paying his subscription shall be entitled to receive the publications of the Association of the current year. The subscription shall date from the 1st January in each year.

The names of new members should be forwarded to the Secretary, Dr. P. H. WILLIAMS, Worcester; or to any of the Secretaries of the Branches.

British Medical Journal.

SATURDAY, FEBRUARY 14TH, 1857.

THE NEW COUNTERBLAST TO TOBACCO.

THERE is a class of persons who employ themselves with all the energy of despair in raising some cry of alarm, and making everybody about them unnecessarily uncomfortable. They parade their bugaboo with a desperation which ensures a temporary public attention, and, as soon as this dies out, they start another of a still more attractive appearance. The vegetarians would reduce mankind to live upon sky-blue and an apple, or at best an egg; the Maine Law liquor men would legislate all spirituous and malt liquors off the face of the earth; and now we are to have an anti-tobacco-smoking agitation, which is to end in the entire demolition of the "Stygian weed". It is quite clear that this restless class of individuals will not "let a body be", and we may think ourselves lucky if hereafter we are not reduced by them to have our diet regulated by act of Parliament. The anti-tobacco-smoking agitation is the last issue of the not very unpleasant brood, and we believe we are indebted to Mr. John Lizars, of Edinburgh, for the hatching, rearing, and sending it forth in a little pamphlet, termed *Practical Observations on the Use and Abuse of Tobacco*, which has already gone through six editions. Upon taking up this publication the other day, we were certainly not a little surprised at some of the statements therein contained, which are calculated, we must confess, to shake the nerves of all smokers already enervated by the abuse of tobacco. The art of making an immediate and startling impression has, however, its drawbacks. Accuracy of detail and sequential reasoning must be sacrificed to a breadth and startlingness of effect. When the urchin sets up his turnip-ghost in the churchyard, he cares little for the Phidian accuracy of its countenance, so long as its goggle eyes at once prostrate Tommy as he comes round the corner.

This little work of Mr. Lizars is open, we fear, to hostile criticism in this direction, as we shall endeavour to point out. Of course it would not be worth the while of the writer to show that the habit of smoking affects only a few isolated individuals, so he at once elevates it into a great and pregnant cause of the destruction of peoples and empires. "Excessive smoking has had no small share in degenerating Spain," he tells us in the preface to his fourth edition. Portugal and Germany are pretty nearly as bad as Spain; but Turkey is "a gone coon", all along of tobacco smoke.

"There can be no doubt, from what has occurred in the war just ended, that, had the Turks never indulged in the vicious habit of smoking tobacco, they would not have required the assistance of the French, Sardinians, and British. They would have been as powerful as in the days of the Sultans Othman, Orchan, Amurath the First, and Bajazet; and would have sent such a message through Menschikoff to the Czar Nicholas as the Sultan Bajazet said to the Count de Nevers of France, when taken prisoner, after his celebrated unsuccessful cavalry charge (like that of Balaklava) near Nicopolis."

Thus the better part of Europe and Asia has sunk from its high estate, chiefly in consequence of this vile weed! When Gibbon toiled and *smoked* and thought over the *Decline and Fall*, in his little garden at Lausanne—when he penned the last line of that remarkable work, and looked out upon the calm lake—he little thought how the labours of future historians would be simplified by the discoveries of science. Even his prophetic eye could scarcely have penetrated to the time when the philosopher, instead of looking to the passions of men working through ages as the causes of the decay of peoples, should seek for them in the narrow compass of the pipe he haply smoked! Mr. Lizars is undoubtedly entitled to the thanks of all future slaves of the lamp for so grand a discovery. Nevertheless, we must remind him that nations were just as apt to degenerate in the patriarchial times, before tobacco-smoke was known, as nowadays.

Tobacco, according to Mr. Lizars, having slain so many nations in the Old World, he anticipates a new victim in the West: at least, he quotes with approval an extract from the *Spectator*, in which the Yankees are described as "undergoing a process of physical and moral degeneracy"; and further, that "the people are like medlars—rotten before they are ripe"—principally in consequence of "smoking and chewing tobacco to excess". Now, we always imagined that the defect of the Anglo-American character was an extravagant go-a-headism—an uncontrollable energy which would stop at nothing. If this effect is caused by tobacco-smoking, we should like to be informed by Mr. Lizars how the same weed can have the effect of steeping the Turk in apathy and idleness! The narcotic must be affected by longitude in some very remarkable manner, which we should like to hear explained.

Mr. Lizars having desolated and overthrown empires with a whiff of smoke in this remarkable manner, we expected to find a fitting figure for his grim landscape. Here he is—a monster such as Frankenstein never could have imagined.

"I once travelled with a gentleman from South America, who first filled his nostrils with snuff, which he prevented falling out by stuffing shag tobacco after it, and this he termed 'plugging'; then put in each cheek a coil of pigtail tobacco, which he named 'quidding', in this country called 'chewing'; lastly, he lit a Havannah cigar, which he put into his mouth—and thus smoked and chewed, puffing at one time the smoke of the cigar, and at another time squirting the juice from his mouth, as so graphically described by Dickens in the boat story, on its way to the Far West. This gentleman was as thin as a razor, with an olive-coloured countenance, and frightfully nervous."

What a creature! Imagine shag tobacco hanging from his nostrils as long as the King of Sardinia's moustaches! But this, the reader will say, is simply an allowable exaggeration, in keeping with the previous picture. Not a bit of it. Here are his own consecutive remarks upon it:—

"The preceding is neither a caricature, nor an exaggerated account of the fearful extent to which the use of tobacco is carried, not only in Europe, as we know, but as there is every reason to fear, in every quarter of the globe, where it either grows, or is unhappily conveyed."

Pray remember, good readers, Mr. Lizars deliberately affirms that *the preceding is neither a caricature, nor an exaggerated account!*

So much for the accuracy of Mr. Lizars' descriptions as a philanthropist. They are somewhat inconsequential, and over coloured, it must be admitted; but still, when gentlemen deal with large problems out of their own direct line, allowances may be made for them. But let us now listen to what he says with respect to tobacco as a direct agent in propagating loathsome and disgusting surgical diseases:—

"Many cases of syphilitic virus, introduced into the healthy constitution by smoking a cigar or pipe, used by a diseased person, have come under my notice. The practice is by no means uncommon, in some ranks of life, for two individuals to smoke the same pipe or cigar alternately, the one taking a puff or *draw* after the other, and in this way the morbid poison produces a similar effect to what is exemplified in the communication of yaws or sibbens, by drinking out of an infected cup or vessel. I have often been consulted by gentlemen having marked syphilitic ulcerated throat, which they could not account for, having had no primary symptoms on the genitals. On interrogating them, they have admitted lighting a pipe used by another, or having accepted of a puff of a friend's cigar."

Labouring men may pass round the cutty pipe, but we never heard of gentlemen smoking a cigar alternately. However, we will take Mr. Lizars' word for it, that some people are dirty enough to do so, and that the virus of syphilis is thus passed on with it. What does this prove, however? That tobacco is therefore pernicious? Certainly not. He might as well, on the same plea, object to water-closets or drinking-glasses. Tobacco not only inoculates the blood with a deadly poison, but it is, according to Mr. Lizars, the cause of carcinoma in the tongue! some horrible examples of which he gives in the book before us in excessively highly coloured plates.

"I have received several communications from professional friends, strongly indicating the strength and extent of medical testimony against the use of the poisonous weed, and out of these I have selected one sent to me by a physician who has long enjoyed extensive opportunities of witnessing the very prejudicial effects which tobacco smoking exercises on the digestive organs. 'In the course of my professional experience', he writes me, 'two or three cases of decided carcinoma of the under lip, all of which terminated fatally, have come under my care, and which could be unmistakeably traced to a sore, occasioned by a burn from a hot cutty pipe'."

Now this extract, from the friend's pen, may be translated to mean nothing more than what has long been suspected but never proved, namely, that the sore created by the corrosive action of a tobacco-pipe may act as an exciting cause in producing epithelial cancer in a person so predisposed. Even here tobacco is only an accidental offender; but at page 14 Mr. Lizars, in commenting upon a case of cancer which had so arisen, would seem to set the weed up as the *cause* of cancer.

"Here I may remark, how many narrow escapes of having cancer of the tongue must *every smoker* have had, when we

consider that every one with a disordered stomach has had one or more pimples on his tongue, which, had they been irritated with pungent tobacco smoke, as in Dr. B——'s case, would in all probability have ended in ulceration, becoming cancerous, and ending fatally."

Does Mr. Lizars mean to aver that cancer may be induced in *every* smoker who happens to have a pimple on his tongue, which the pungent tobacco-smoke irritates? If this is his position, our inquiries into the causes of carcinoma have received an immense impetus. We fear, however, that our author means no such thing. No other interpretation can be put upon his words, however; and we therefore draw attention to the statement, in order that it may be either disavowed or more deliberately affirmed. We are far from saying that tobacco-smoking is at all times a wholesome habit, or one that can be indulged in with impunity by every individual. That it has an injurious effect upon the nervous systems of individuals out of health, and on those of a particular idiosyncrasy, there can be little doubt; but so also has tea. Indeed, we believe that green tea, universally adulterated as it is, has much more to answer for in producing nervousness than tobacco. Be that as it may, however, what we wish to insist upon is, that glaring exaggerations will never serve the truth; and we feel convinced that not one cigar the less will be smoked in consequence of the highly coloured statements of Mr. Lizars, to whom we would apply his own phrase with respect to tobacco, only a little altered—*Throw away exaggeration for ever.*

We shall return to this question.

TESTIMONIAL TO WILLIAM NEWNHAM, ESQ.

IT is with feelings of the greatest pleasure that we are enabled to announce that it is proposed to make a public recognition of the services of the excellent and indefatigible founder and treasurer of the Medical Benevolent Fund. From the period of the establishment of the Benevolent Fund—"that jewel in the crown of our Association"—in the year 1835, Mr. Newnham has been, as is well known, its animating and guiding spirit. It is now proposed to purchase immediately, and present to Mrs. Newnham, the capital portrait of Mr. Newnham by Mr. James Andrews, which was exhibited last summer in the rooms of the Royal Academy. In order that the opportunity of paying this compliment to Mr. Newnham may be enjoyed by many, it has been determined that no subscription shall exceed one guinea. In carrying out this project, a Committee has been formed, consisting of the following gentlemen:—

Sir JAMES CLARK, Bart., *President of the Medical Benevolent Fund Committee.*
JAMES BRIGHT, M.D., *Secretary to the Medical Benevolent Fund Committee.*
JOSEPH TOYNBEE, Esq., *Assistant-Treasurer to the Medical Benevolent Fund Committee.*
Sir CHARLES HASTINGS, M.D.,
Sir JOHN FORBES, M.D.,
JOHN CHURCHILL, Esq.,
} *Members of the Medical Benevolent Fund Committee.*

Sir John Forbes has kindly consented to act as Secretary and Treasurer of the Testimonial Committee; and it is requested that all subscriptions and inquiries be addressed to him, at 12, Old Burlington Street, London.

THE WEEK.

THE Representatives of the different Universities and Colleges of Great Britain and Ireland have held several meetings during the week in the Council Chamber of the College of Physicians, at which the different provisions of the Medical Reform Bill to be laid before Parliament were agreed upon, with the single exception of the number of independent members to be nominated to the Council. We are given to understand that "the parties to the Bill" consider that, as the power of nomination is to be given to the Government, it would be only respectful to leave the numbers to be selected to its consideration. We state this much, in order that the associates may be placed in full possession of the present state of affairs. We leave the rest to the careful watching of the Reform Committee, which will, we doubt not, take care that the general interests of the profession are duly considered during the passage of the Bill through Parliament.

From intimations in the public press, we are led to believe that the subject of Sanitary Legislation will receive a good share of the attention of Parliament during the present session. One of the daily papers (the *Morning Post* of February 11th) says:—

"It is proposed, we learn, on an early day, to bring in a Public Health Bill, continuing the Board of Health for five years, and giving to it and to local boards powers for regulating in a sanitary point of view all that pertains to sewerage, the cleansing of streets, houses, and factories; the supply of water to towns, the police of slaughter-houses, the regulation of burials, the formation and maintenance of pleasure-grounds, and the management of public baths and washhouses. The Bill to be introduced will be in accordance with the recommendations of the Select Committee; and will, it is supposed, embody the results of valuable evidence and thoughtful deliberation."

We trust that the news may be true; and shall earnestly look for the appearance of the proposed Bill, in order that we may have an opportunity of making more extended comments on the important subject with which it deals.

The body of Mrs. Ann Bacon, mother of Thomas Fuller Bacon, at present in custody on the charge of murdering his two children, has been exhumed at Stamford, in consequence of a suspicion that her death, which occurred in May 1855, was caused by arsenic administered by her son. An inquest has been instituted, at which the fact that Bacon bought arsenic in Stamford at about the period of his mother's death, and other circumstantial evidence in support of the suspicion, have been brought forward. The viscera have been placed for analysis in the hands of Professor Taylor, who was present at the exhumation of the body. Pending his report, the inquest is adjourned till Wednesday next, the 18th instant.

A Meeting of the Committee of the Poor-Law Medical Reform Association, numerously attended by members from various parts of the country, was held at 37, Soho Square, on Tuesday last, R. Griffin, Esq., in the chair. Several letters were read from noblemen and other members of Parliament, expressive of their sympathy and willingness to cooperate with the Committee for an improved system of poor-law medical relief. The present position of the question having been considered, it was resolved to convene a general meeting of the union medical officers, and other members of the profession, to petition the Legislature for a redress of the grievances complained of. A sub-committee was appointed to frame a petition for presentation to the House of Commons, as well as one to be signed by the magistrates, clergy, and other ratepayers, who feel that the present system is unjust both to the poor and to the medical officers.

Reports of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, January 27th, 1857.

Cæsar Hawkins, Esq., F.R.C.S., President, in the Chair.

ON DISPLACEMENT OF THE SCAPULA UPWARDS THROUGH PARALYSIS OF THE SERRATUS MAGNUS MUSCLE, AND CONSEQUENT RETRACTION OF THE RHOMBOIDEI, LEVATOR ANGULI SCAPULÆ, AND TRAPEZIUS MUSCLES. BY BERNARD E. BRODHURST, ESQ.

The author stated this to be a very rare affection. The patient was sixteen years of age, tall and robust. The right shoulder was two inches higher than the left; and the inferior angle of the right scapula was five inches higher than that of the left side. The postero-superior angle of the scapula projected immediately beneath the skin on the anterior surface of the neck, one inch and a half above the clavicle. Immediately above this point, the trapezius formed a thick prominent cushion. The serratus magnus muscle of the right side could not be distinguished even during forced inspiration. The motions of the right arm were limited—that is to say, the elbow could be raised only seven inches beyond a right line with the trunk, and violent movements of this arm occasioned pain, in consequence of the projection of the scapula. Paralysis of the serratus magnus muscle had been produced when the patient was two years old, by her being caught by the arm when falling from the arms of a relative. Weakness of the limb was observed soon after the accident; and in the course of some months the shoulder was observed to be unduly prominent. A weight of five pounds was fastened upon the shoulder, and this was subsequently increased to eight pounds, which was worn during several years. The rhomboid muscles, the levator anguli scapulæ, and portions of the trapezius muscles, were tensely retracted. These muscles were divided subcutaneously; and after the fracture had healed, pressure was made above the spine of the scapula, to endeavour to depress that bone. This was so far successful that the normal position of the scapula was in great measure regained, the motions of the shoulder were rendered more free, and pain on motion was entirely removed.

Mr. Lonsdale did not agree with the author as to the rarity of paralysis of the serratus magnus muscle. He had previously seen cases of the kind; and others were quoted by Dr. Laycock, in his work on the *Nervous Diseases of Women*. He believed the elevated position of the scapula depended a good deal on curvature of the ribs at the upper part of the thorax, the existence of which was indicated by the cast, which showed a convexity on one side, and a concavity on the other, with a marked spinal curvature existing also. The success of the operation he would not dispute, not having seen the patient.

FIVE CASES OF TRACHEOTOMY IN CROUP, WITH REMARKS ON CERTAIN POINTS CONNECTED WITH THE OPERATION. BY HENRY WM. FULLER, M.D., F.R.C.P.

The author began by narrating the particulars of five cases of inflammatory croup, for the relief of which tracheotomy was performed in St. George's Hospital. In each instance, the operation was deferred until the last stage of the disease, when every remedy had failed, and death was imminent. In two of the cases, the operation was successful in saving life; in three, it failed of its object. Four of the patients coughed up a considerable quantity of mucus or false membrane through the artificial opening, and received immense relief from the operation; whilst the fifth was nearly moribund at the time of its performance, and expired almost immediately afterwards. Dr. Fuller remarked, that the success thus obtained is highly satisfactory; and that, unless these recoveries are quite exceptional, tracheotomy ought to be had recourse to when other remedies have failed. He admitted, however, that the inferences derivable from such a limited number of cases are not of themselves sufficient to determine the propriety of the operation, and he therefore proceeded to discuss the question generally, and endeavoured to bring together such facts as should lead to a decision—1st, as to whether the operation of tracheotomy is justifiable in any case of croup? 2nd, if so, under what conditions, and at what stage of the complaint? 3rd, whether the existence of certain symptoms or other circumstances ought not to cause us to hesitate in recommending its performance? 4th, whether any, and what, medical treatment is necessary after an opening has been made into the trachea? With a view to a correct appreciation of the subject, Dr. Fuller began by referring to the difference existing physiologically and pathologically between idiopathic inflammatory croup and the diphtheritic form of the disease which commonly prevails in France, and he pointed out that the objection usually urged against French statistics of tracheotomy in croup—viz., that diphtheritic cases are much more favourable for the performance of the operation than are the croup cases usually met with in this country—has no foundation in fact. He called attention to the circumstance that diphtheritis is often accompanied by glandular swellings in the neck and œdematous fulness of the throat, whereby the operation is rendered much more difficult than in inflammatory croup; and that the type of the accompanying fever is so low as often to destroy life, quite independently of any affection of the air-passages. He then proceeded to show, by reference to 483 cases in which tracheotomy has been performed for the relief of croup in France, that the operation has proved eminently successful in the hands of French surgeons; and he reminded the Society that, inasmuch as the condition of the throat externally, and the nature of the accompanying fever in diphtheritis, are by no means favourable to the operation, the success which has attended it can be explained away only on the supposition, often put forward by English writers, that in France the disease seldom extends into the trachea and bronchi, and is rarely accompanied by bronchitis or pneumonia. The fallacy of this supposition he then proceeded to demonstrate by reference to the writings of French authors, and to the recorded results of the *post mortem* investigation of 311 fatal cases of croup in France, and he showed that in regard to its pathological effects, diphtheritis, when accompanied by croupal symptoms, does not, as compared with inflammatory croup, present any greater prospect of success for the operation than it does in the character of its accompanying fever, or the condition of the throat externally. Having thus established the success of the operation in the hands of French surgeons, and the absence of any special cause for that success, Dr. Fuller proceeded to inquire into the circumstances which have led to the disrepute of the operation in England. These he traced to theoretical objections founded on the pathological results of the disease, to the almost unanimous and unqualified condemnation of the operation pronounced by successive English writers, and to the ill success which had attended the operation in the few cases in which it had been practised prior to the publication of their respective works. He insisted, however, that theoretical objections are of little value as compared with the results of practical experience; and he therefore appealed to that source for information on the subject. He showed, by reference to statistics derived from the Hôpital des Enfans Malades at Paris, that whereas, out of the first 100 cases operated on at that institution, one only recovered, a more extended experience in the mode of performing the operation, in the precautions requisite to ensure success, and in the carrying out of the necessary after treatment, has led, since 1850, to the saving of 47 out of 215 cases; or, in other words, to the rescuing from death of nearly one out of every four patients. He argued thence against those persons in this country who condemn the operation simply because it is opposed to their preconceived notions, or has proved unsuccessful in the few instances of which they are personally cognisant. Further, he showed that, even in Great Britain, the recorded results of the operation afford a fair amount of success. Twenty-two cases only have been recorded in England, and in no less than eight of these life was saved by the operation; and, although doubtless many unsuccessful cases have occurred which have not been placed on record, still Dr. Fuller argued that, if life can be saved by operative interference, even in a small proportion of instances, the chance afforded by the operation ought not to be withheld where all other means have failed, except under some peculiar circumstances. Dr. Fuller next proceeded to dispute the propriety of having recourse to tracheotomy at an early stage of the disease. He showed that patients, in the second stage of croup, will sometimes recover under proper medical treatment, even when those symptoms appear to be desperate; and, on the other hand, that the theoretical objections commonly urged against deferring the performance of tracheotomy until the third stage of the disease, have no foundation in fact. In proof of this, he appealed to the results of the five cases which have fallen under his own observation at St. George's Hospital, as also of many of the other cases on record; and, further, to the corroborative evidence afforded by the recent change of opinion evinced by MM. Trousseau, Bretonneau, and others who formerly were most zealous in their advocacy of an early per-

formance of the operation, and who now defer it until a much later stage. Dr. Fuller condemned the indiscriminate performance of tracheotomy in croup. So much danger and difficulty attend the operation, even in favourable cases, that he considers it almost necessarily fatal if the patient is of very tender age, or has been out of health prior to his attack of croup; if his illness has been preceded by pneumonia or severe bronchitis; if he is suffering from exanthematous or other disorder; and, further, if he is in such a position of life that his parents are unable to secure for him proper skilled attendance night and day. He spoke of the gradual sinking sometimes observed in fatal cases, many hours after the operation, whilst all the symptoms are progressing favourably, as analogous to the sinking which occasionally takes place, under similar circumstances, in persons who have been partially asphyxiated; and he attributed it in part to pulmonary collapse, and in part to nervous exhaustion consequent on the long continued struggles for breath. He referred to the use of tracheal tubes of too small a calibre, or of improper construction, as one great cause of the failure of the operation; and, as another, to the neglect of proper after treatment, or to the administration of improper remedies. He pointed out that, in almost all the fatal cases on record, wine and brandy had formed the chief, if not the sole, medicaments; whereas, in almost all the successful cases, calomel, antimony, and the measures which are considered useful before the operation, were steadily persevered in afterwards; and he called attention to the fact, that the depression which accompanies the last stage of the disease, in which alone he recommends the operation, is the depression of asphyxia, which is to be relieved by the free admission of air, and not by the administration of stimulants. He recommended that the trachea-tubes be made somewhat larger, shorter, and less curved than those in common use; that the outer canula be shorter than the inner one, and that both be of the same diameter from one end to the other, but that the outer one, instead of being made of one piece as at present, should be divided longitudinally into two blades, flattened towards their inferior extremity, so as to come into close apposition, and to admit of easy introduction into the trachea. These blades should be made to open like the blades of a dilating bivalve speculum, so as to admit, when fully expanded, an inner tube of uniform diameter throughout. This arrangement could not only conduce to keeping the inner tube clear of mucus, but would render serious obstruction to the respiration well nigh impossible, inasmuch as, if the inner tube were to be clogged in any way, and the extremity of the outer canula were to be also choked with mucus, the chink existing between its expanded blades would provide a free passage of air immediately on the withdrawal of the inner tube.

Dr. WEST wished to set Dr. Fuller right upon one point in his paper, from which it would appear that he (Dr. West) was altogether opposed to the operation of tracheotomy in cases of croup. In his lectures, he had advanced certain facts to show why the operation was more successful in France than in this country. He had said that the objections to the operation advanced by Dr. Cheyne and others were not satisfactory. He had further said, that amongst other causes of the non-success of the proceeding was the neglect of precautionary measures, and the inattention to the administration of medicine and to the preservation of an equable temperature both before and after the operation. Still, his own experience was not favourable to the proceeding. Ten cases had been operated on under his immediate superintendence, and in no instance was the operation followed by a good result. In all these cases it had been resorted to sufficiently early—that was, before the patient was in such a condition that death was imminent from the disease; and was skilfully performed. The patients were placed in a favourable position in regard to temperature, had the advantages of the attendance of a special nurse, and every attention was paid to them. With regard to the size of the canula to be employed, similar remarks to those made in the paper had been also advanced by M. Trousseau. He had instanced a person breathing through a quill placed in the mouth. At first, the breathing was easy; but the supply of air soon became inadequate; and it was necessary to throw the quill away, and to take in a sufficiency of air to support respiration. M. Trousseau had insisted upon the necessity of a large canula, and with this opinion he (Dr. West) concurred. The last publication of M. Trousseau indicated that his opinions had undergone a change in reference to this operation. He would not now resort to the proceeding so frequently nor so early, but would limit it to those cases which Dr. Fuller had indicated as proper for the operation—namely, in the latter stages; and in this opinion he (Dr. West) agreed. He still thought that cases of diphtheritic inflammation extending to the larynx were more favourable to the operation than cases of true croup; for in the first the mischief travelled downwards, whilst in croup there were usually complications, such as bronchitis, etc. In most cases of croup, the symptoms were ushered in with catarrh and fever, and the laryngeal symptoms went on *pari passu* with these. Of thirty cases of croup which he had examined after death, in by far the larger number of cases the disease was not limited to the trachea or larynx, but the larger and even smaller bronchi had become affected, and in some instances the substance of the lung itself was diseased. These symptoms, he believed, went on in spite of the temporary relief afforded by tracheotomy. Notwithstanding all these drawbacks to the operation, and the general non-success of its performance in England, he should still recommend it in a large proportion of such cases as those indicated in the paper of Dr. Fuller.

Dr. WEBSTER remarked, that in all the boys mentioned by Dr. Fuller, and on whom tracheotomy had been performed, the operation proved unsuccessful; whereas, the two cases which recovered were girls, aged five years and upwards. This coincided with the ordinary termination of croup, which was usually most fatal in patients from two to three years of age and under; besides being oftener followed by death in male than in female infants. Dr. Webster believed these circumstances influenced considerably the success of tracheotomy in croup. The author had quoted also an expression implying that croup was not very common in Paris. This he thought erroneous. The disease prevailed very frequently in that capital, the mortality thereby being double the ratio of London, although recently it had been much more fatal here than ten years ago. In France, tracheotomy was oftener successful than in England. This, Dr. Webster considered, partly arose in consequence of the operation being generally performed at an earlier stage; as also, he could not help believing, from some of the cases being instances of diphtherite. When at Tours, several years since, he had heard read an important paper, by M. Bretonneau, strongly advocating tracheotomy, and speaking of its marked success in croup; but, since then, that celebrated practitioner, it was said, had somewhat modified his opinions. Still there was no question that opening the windpipe in tracheal diseases was more commonly employed in France than in this country, and with better results.

Dr. FULLER was glad to know that the opinions of Dr. West substantially coincided with his own. He still thought, however, that the opinions which had been expressed by eminent authorities against the operation, had made practitioners hesitate to resort to it. These opinions had influenced himself, and in the first case, as related in the paper, he was induced to resort to the operation, upon the remark of a bystander that it was the only chance for the patient, and that he must inevitably die unless this proceeding were resorted to. He (Dr. Fuller) agreed with Dr. Webster as to the difficulty and danger of performing the operation on very young children—difficulties which arose from the fits of dyspnœa which come on, and from the necessarily small size of the canula which must be used. The great number of successful cases in England were performed on children from four to seven years of age.

MEDICAL SOCIETY OF LONDON.

SATURDAY, JANUARY 24TH, 1857.

W. D. CHOWNE, M.D., President, in the Chair.

INSUFFICIENT SIZE OF THE OPENING IN TRACHEOTOMY.

BY EDWARD SMITH, M.D.

Dr. EDWARD SMITH drew attention to the insufficient size of the opening usually, or perhaps invariably, made in tracheotomy. He thought it very probable that the ill success of the operation might in part be attributable to that circumstance. In many cases there is constant relief afforded on the opening having been made, and then soon afterwards that relief disappears; and he accounted for it by showing that when the glottis was very nearly closed, as is commonly the case before the operation is performed in this country, any additional volume of air, however small, would afford relief; but unless that volume were nearly as large as that commonly passing through the healthy glottis, it would be insufficient to sustain life. It would not be a material circumstance if the glottis were not quite closed; for the presence of the tracheotomy tube in the trachea would so impede the passage of air

through the glottis, that probably no air would enter the bronchi but that passing through the artificial opening. The size of the opening should therefore be that of the glottis; but since the area of the glottis constantly changes with respiratory movements, it cannot be estimated and taken as a guide. The trachea, however, has a tolerably constant size, and must have an area corresponding with that of the glottis above and the bronchi below, and thus it may be taken as a safe guide for this purpose. In an adult, therefore, he would recommend that the clear opening should not be less than five-eighths of an inch, and in a child not less than three-eighths to half an inch, according to its age. In an adult it must be competent to pass twenty to thirty cubic inches, and in an infant of two years old from twelve to fifteen cubic inches at each inspiration. If the area of the opening be less, then the patient would not live, although the operation might be very proper, and fitted to succeed with a large opening.

THE MEDICINAL EFFECTS OF AMMONIA AND ITS PREPARATIONS.

BY T. OGIER WARD, M.D.

Ammonia had never been considered to be a normal constituent of the blood, as its presence had not been detected except after death in cases of typhus, cholera, melæna, and other diseases of a putrid character, until Dr. Richardson's recent discovery that healthy blood owes its fluidity to the presence of ammonia, which is given off during its coagulation, which process may be arrested, and the fibrine re-dissolved, by the restoration of the alkali. An interesting inquiry here suggests itself: how does the ammonia escape from the body during the coagulation of the blood, and how is it retained in those instances in which the blood remains fluid after death? Assuming the truth of Dr. Richardson's views, Dr. WARD had examined and compared the therapeutic effects of the various preparations of ammonia. He had found that, whether applied externally or taken inwardly, they possess in common the property of dissolving the proteine elements of the blood, whether in the vessels or effused into the tissues. This similarity in the effect of ammoniacal medicines is owing to their ready decomposition, the ammonia being separated, and forming the chief curative agent, though it is aided by the other substances originally combined with it. Thus its stimulant and solvent action is similar in kind, if not in degree, when used either externally or inwardly in the form of gas, liquor ammoniæ, or combined with carbonic acid, etc. From the utility of these preparations in the treatment of venomous bites and stings, inflammatory swellings, diphtheritis, croup, etc., we may suppose that they will be equally efficacious in urticaria, erythema nodosum, and erysipelas, in which there is an effusion of the fibrinous elements of the blood. In these and other inflammatory diseases and conditions, it is probable that the benefit of the salts of ammonia is owing to their preventing or removing the effusion of fibrine from the inflamed parts. In this way, although the primary action of ammonia is stimulant, its remote effects are sedative or debilitant, as it not only arrests inflammatory action, but, by its resolvent and secernent power, carries the products of inflammation out of the system, and hence its utility in all active febrile complaints. It is to this attenuating property that its use as an antidote to drunkenness and to the stupor of opium is to be ascribed. Its stimulant powers are of use in poisoning by hydrocyanic acid, in the cold stage of ague, and in the retrocession of gout, rheumatism, and the exanthemata, as well as in syncope, hysteria, epilepsy, and convulsions. The hydrochlorate, which is the least easily decomposed, is probably the most useful of the salts of ammonia, as it not only possesses the stimulant, resolvent, and secernent properties of the others, but owing to its combination with chlorine, is endued with tonic powers, by which its prolonged use, unlike that of the other preparations, it attended with invigorating effects both to mind and body; and thus it forms an excellent substitute for mercury in cases where this medicine is inadmissible from its tendency to produce cachexia.

PATHOLOGICAL SOCIETY OF LONDON.

TUESDAY, JANUARY 6TH, 1857.

J. M. ARNOTT, Esq., President, in the Chair.

ABDOMINAL ORGANS CONNECTED TOGETHER BY COLLOID AND ENCEPHALOID DISEASE, ASSOCIATED WITH SCIRRHUS OF THE PYLORUS. BY W. O'CONNOR, M.D.

THE parts were removed from a man, aged forty-six, a native of Milan, by trade a picture-frame maker, who was admitted into the Royal Free Hospital on the 20th of October last. When first seen, he complained of severe pain in the epigastric region, rather to the right side, with oppressed and difficult breathing, the abdomen was swollen, and he was slightly jaundiced. The pulse was small and frequent—110 in a minute. The patient was rather thin, and had an anxious expression of countenance. On examination of the chest there could not be detected any evidence of disease of the lungs or heart. On the 23rd the symptoms were very much relieved by the treatment adopted, which consisted of purgatives, composed of compound powder of jalap and calomel (the bowels being very much confined) and diuretics, with hydrocyanic acid and sesquicarbonate of soda. On the 30th the symptoms became more urgent, the pain in the epigastrium was excruciating, and there was general dropsy, with jaundice. Mild mercurial treatment, with moderate purgatives and diuretics, together with a blister to the painful part, were had recourse to; and by the 7th of November the dropsy and jaundice entirely disappeared, the only symptoms existing were pain in the epigastric region, but to a trifling extent only, and tenderness on pressure to the left of the umbilicus. The great size of the abdomen being now reduced, on a careful examination a hardness was felt occupying nearly the whole of the epigastric region, extending from the under surface of the liver to the left hypochondriac region, from thence descending to and filling up the left iliac fossa. On inquiry, it was found that the patient had suffered more or less for nearly two years, from severe pain in the epigastrium, with frequent attacks of nausea and retching, and that he had once before had an attack of jaundice. He was, during these attacks, alternately an out-patient at St. Bartholomew's Hospital, and under the care of Mr. Pollock, of Hatton Garden. Mr. Pollock attended him some months back for an enlargement of the right testicle, which ultimately did well. Dr. O'CONNOR now viewed the case as one of malignant disease, which he supposed to be primarily connected with the stomach, and from thence extending throughout the abdomen.

On the 18th of November, the patient complained of more severe pain all over the abdomen, intense and frequent thirst, constant retching, pain, and starting of the left lower extremity; the pulse was 140; the urine scanty, and loaded with urates; the tongue furred, skin hot, and the bowels constipated. Effervescing medicines, with anodyne fomentations and mild purgatives, and afterwards anodynes internally, were had recourse to with benefit. In a few days the patient vomited a quantity of fœtid purulent matter free from fæces and the coffee-ground character usually present in these cases. The urgency of the symptoms was now very much lessened: the enlargement in the left iliac fossa became less, and the starting and painful condition of the left lower extremity nearly disappeared; the tongue was clean, and the urine was clear and abundant. On further examination of the abdomen, nodulated masses could be felt distinctly over the whole of it. The retching became more frequent, and nothing could be retained on the stomach; even a teaspoonful of cold water or a little ice gradually dissolved in the mouth was rejected as soon as it reached the stomach, and it was necessary to sustain the patient by enemata of brandy and beef-tea. During this time there was frequent vomiting of the same purulent matter as before. In this state the patient continued until the 16th of December, when he died a mere skeleton.

A *post mortem* examination was made thirty-six hours after death. Nodulated masses could be distinctly felt through the abdominal walls. On opening the abdomen, which contained about three pints of serum, and reflecting the parietes, the following structures presented themselves, as seen in the model and drawing, from above downwards. The thickened diaphragm; the ensiform cartilage displaced upwards, and to the right side; a small portion of the left lobe of the liver; a nodulated mass in the situation of the omentum, concealing all the other structures, excepting the left iliac fossa, where a small portion of the sigmoid flexure of the colon touched the abdominal parietes. The reflected parietes presented nodules of colloid cancer. The organs of the abdomen were removed *en masse*, being all connected together by a large apparently colloid and encephaloid growth. Their relations to each other were altered in various ways by pressure of the growth, which had not infiltrated their structure. Thus the pyloric extremity of the stomach was dragged down, so that the longitudinal axis of that organ was more vertical than usual. When opened and viewed posteriorly, a large mass of cancer was seen, the central portion of which was scirrhus and the peripheral portion colloid. The mucous membrane of the pylorus was to a small extent destroyed. At this part there was a very small opening, which appeared to be recent. The peritoneal

surface of the stomach presented nodules of colloid, but the mucous membrane was entire, except at the pylorus. The liver, especially the right lobe, was much compressed by the diaphragm, which was itself much thickened, throughout its entire extent, by a layer of cancerous deposit; this layer had not superseded the muscular and tendinous structure of the diaphragm, although, from pressure, it would appear that the muscular portion was atrophied. The right lobe of the liver reached up to nearly the third rib. The inferior cava and hepatic duct were surrounded and compressed by the general mass, and the gall bladder was therefore much engorged. The structure of the spleen was condensed from pressure of the surrounding cancer, which had encroached into the hilus, with which it accurately corresponded; but the interior of the spleen was free from cancer and otherwise healthy, excepting two buff-coloured spots, the nature of which could not be made out. The kidneys and suprarenal capsules were healthy, but the latter were compressed and atrophied. The pancreas was identified with the general mass, which extended downwards throughout the omentum; and the transverse colon lay in a groove surrounded, but not infiltrated, with cancer. The remaining portion of intestines, large and small, were free and unobstructed; but a few convolutions which lay across the lumbar vertebra were reduced almost to the size of a quill. The bladder was healthy, and so were the lungs and heart, but the veins of the latter were very turgid. On microscopical examination, the growth proved to be made up of schirrus, colloid, and encephaloid.

Dr. O'Connor observed that there are two cases recorded in the "Transactions of the Pathological Society of Dublin," one by the late Dr. Greene and the other by Dr. Lees, very much resembling the case brought forward by him. In Dr. Greene's case, however, there was no retching or vomiting until about ten days before death, when a coffee-coloured fluid was thrown up; but there was ulceration of the stomach. The patient had been complaining nine months. In Dr. Lees's case the patient had complained only for five weeks; there was no pain or tenderness on pressure; there was frequent vomiting, but no ulceration of the mucous surface of the stomach. In the current number of the *Glasgow Medical Journal* there is a similar case recorded by Dr. J. Bell, occurring in a woman thirty years of age, who died in eight months after the first symptom was complained of. In Dr. Bell's case, besides the pylorus being a mass of scirrhus, and the omentum being also attacked, the cancerous matter infiltrated some of the abdominal organs.

Dr. BRINTON said he had examined portions of the diseased masses of these specimens. The yellow, transparent, jelly-like, and tremulous nodules which occupied the peritoneum, had all the characters of colloid or alveolar cancer—consisted, that is, of a sparing, delicate fibrous network, enclosing in its meshes cells of equal delicacy. Some of these cells contained highly-refractive (oil?) globules; a few, pigment; many, cytoblasts. The stomach offered appearances which were interesting as an example of a condition which he (Dr. Brinton) thought was not uncommon, and sometimes led to ambiguity of description. Though offering to the naked eye all the characters of scirrhus, the pyloric disease was shown by the microscope to possess much of the structure of colloid, differing from the colloid present in the peritoneum chiefly in the larger quantity of fibrous network between its cell-growth—a complication of the two varieties of cancer which, in some degree or other, was rarely absent from the later stages of these gastric cancers of rapid growth. The diaphragm seemed affected chiefly by its peritoneal covering sharing in the general involvement of this serous membrane; and it was difficult to avoid conjecturing that such a wide spread of the colloid deposit was often brought about by the effusion of a cancerous juice into the peritoneal cavity. The striking immunity of the liver, as well as that of the lungs, kidneys, and other organs, appeared, from his researches, to be rather favoured by diffuse secondary deposits in the peritoneum, like that seen in this case.

EXTENSIVE INVAGINATION OF THE SMALL AND LARGE INTESTINE: POST MORTEM DESTRUCTION OF THE POSTERIOR WALL OF THE STOMACH BY THE GASTRIC JUICE. BY J. H. BALLER, ESQ.

The subject from whom this specimen was taken was a female infant, aged six months. She had been fed at the breast entirely until the last week of her life, during which she had daily eaten some bread-and-butter. She had never had diarrhœa or taken any aperient medicine, the action of the bowels having always been regular and natural. On the evening of December 30th she was suddenly seized with vomiting and apparently great pain, which continued, but appeared to become easier during the night. Mr. BALLER first saw her about noon on the following day; she seemed then to have pain in the abdomen, vomited occasionally, and passed frequently a small quantity of blood-stained fluid from the anus, but no fæces whatever. The symptoms continued, with little variation, for four days, although the pain seemed to have been modified, probably by the opiate enema which was used daily. On the 3rd of January a larger enema was administered, containing some olive oil and two drachms of castor oil, which, according to the nurse's report, was succeeded by ten stools. There certainly was a little feculent matter on some of the napkins shown to Mr. Baller, and now no blood, but some transparent and glairy mucus. The skin was pale, and the countenance exhibited distress. There was much tenesmus. Death occurred on the morning of January 4th, being the fifth day of the illness. The peritoneum contained a few drachms of brownish serum; the bloodvessels of the small intestine were much congested, the intestine being distended with air. The cæcum could not be discovered; but, on tracing the small intestine, the ileum was found to terminate in the descending colon. The latter was much puckered, and, together with the rectum, was distended to within two inches of the anus, with a volvulus consisting of a portion of the ileum and the whole of the superior part of the large intestine. The volvulus was of a very dark colour, from congestion; and the presenting portion afforded to the top of the finger the same sensation as the adult os uteri. The posterior wall of the greater curvature of the stomach was reduced to a soft gelatinous state, and it allowed the contents of the organ, about three ounces of partially digested milk, to escape on the first attempt to raise it from its position.

HYDATID IN THE ANTERIOR CHAMBER OF THE EYE OF A HORSE. BY J. HUTCHINSON, ESQ.

The eye had been forwarded to Mr. HUTCHINSON by post in an unopened state, and without any note of explanation as to who had sent it, or whence it had been obtained. It was quite fresh, and showed, on section, a most interesting condition. The cornea was thickened and opaque; the vitreous humour was yellow, and of the consistence of syrup, running out when the sclerotic was divided: the retina was detached, and hanging in folds; the lens was hard, and of a yellowish-brown colour. The eye, in fact, although retaining its natural shape, etc., was completely disorganised. The cause of this mischief seemed to be found in the presence in the anterior chamber of an hydatid cyst. This cyst, which consisted of two concentric laminæ of beautiful translucency and extreme thinness, occupied the whole of that chamber, and pushed back the iris. Around its lateral margin it was in several parts firmly united to the adjacent textures. Its contents were a clear fluid and some portions of a solid material resembling isinglass. A careful microscopic examination had failed to detect either echinococci, or that lamination of the cyst-wall which is characteristic of the echinococcous hydatid. Surrounding this outer hydatid cyst, in every part, was a tough membrane—thick and corrugated in parts, in others thin and flexible—which appeared to be the inflamed and thickened membrane of the aqueous humour. It accurately lined the whole of that chamber, and was adherent at its circumference to the ciliary ligament; passing in front of the iris, it dipped through the pupillary aperture to become united to the capsule of the lens. The posterior chamber was so much obliterated by the pushing back of the iris, that it was impossible to say whether or not it were also lined by the membrane alluded to. Mr. Hutchinson adverted to the improbability of this membrane having resulted from inflammatory changes, although in a lost eye such a mode of production must be admitted to have been possible. He thought the specimen militated very strongly against the opinion held by Mr. Bowman and others, as to the non-existence of the membrane of the aqueous humour, and in support of that of the older anatomists.

The specimen was referred to Mr. Haynes Walton, to report upon to a future meeting.

RIGHT DIRECT INGUINAL HERNIA. BY JOHN BIRKETT, ESQ.

A male, aged 81, had been the subject of strangulated bowel about seventy-two hours, when Mr. BIRKETT was called upon to relieve the constriction by operation. He died of acute peritonitis. He had been ruptured for about eight or nine years, according to his statement. The preparation exhibited the characteristics of this variety of hernia very distinctly.

The protrusion extended into the scrotum, the external abdominal ring. The spermatic cord was attached to the outer and inferior border of the tumour, just before it traversed the external ring. The fibres of the cremaster muscle, the conjoined tendons of the internal oblique and transversalis muscles, and the pubic portion of the fascia transversalis assisted to form the coverings of the peritoneal sac. The neck of the sac, viewed from within, was about one inch and a half to the inner side of the internal abdominal ring, and half an inch inside the course of the internal epigastric artery.

Editor's Letter Box.

ENCOURAGEMENT OF HOMŒOPATHY BY LEGITIMATE PRACTITIONERS.

Letter from James Long, Esq.

Sir,—The letter of Mr. A. G. Field in the British Medical Journal of the 3rd ult., in reference to the above subject, accords so much with my ideas in most of the points which he has so ably brought forward, that I have determined, now that the subject has rested for a time, to inform Mr. Field, and through him, those who wish to know the truth of the matter, the circumstances which led to the letter which appeared from Mr. J. R. Humphreys, dated December 3rd, and to the editorial remarks thereon, in the journal in which Mr. Humphreys' letter appeared. I do not find fault with the facts stated by Mr. Humphreys so far as they were known to him, nor yet with the conclusion he might feel disposed to draw from them, but the latter part of his letter indisposed me to enter into any explanation to him, and the editorial remarks founded upon his letter decided me not at all events for a time to give any explanation.

In Mr. Humphreys' letter, a surgeon in Shrewsbury, with whom I am on terms of intimacy, and for whom I have a great esteem, is alluded to: as his name has not appeared in connexion with this subject, and as I shall have occasion to allude to him, and as I conceive I have no right to bring any man's name unnecessarily forward, I shall throughout this statement call him Mr. A.

On the 25th November, just before I left my house in the morning, the following telegraphic message arrived. George Wilkin, Shrewsbury, to James Long. "Can you come this evening to see Mr. Corbet, Sundorne Castle, if required; a surgical case." I telegraphed back, the boy waiting for an answer: "Yes. State what hour, and nature of case." Between five and six in the evening, the following message arrived: "Leave landing stage at 7.20, here by ten. Sleep at Sundorne. Abscess and phagedenic ulcers of heel."

At the time when I received and replied to this message, I had no recollection who George Wilkin was, but in the course of the day was reminded by one of my family that he was the Dr. Wilkin with whom I had been acquainted in Liverpool, and was now settled in Shrewsbury; he had some years ago, in conjunction with Mr. A. of Shrewsbury, assisted me to remove a scirrhous breast

Arriving at Shrewsbury between ten and eleven, I found Dr. Wilkin waiting for me with a car; on our way to Mr. C.'s, a distance of about four miles, I received further information about the case, and on inquiring if I were to meet Mr. A., which from the circumstance just mentioned I had anticipated, he informed me that Mr. A. had been requested to meet him, but had refused, on the ground that he was a homœopathist, which he was not. That Mr. C. had, from circumstances which it is unnecessary to specify, taken a strong prejudice against medicine; that he had been treating him almost entirely on dietetic principles; that the case required, in its present state, the superintendence of a surgeon; and as the family could not obtain the services of Mr. A., they had sent for me.

This led me to remark that I regretted to find myself in such an unexpected position, especially as regarded a gentleman for whom I had a regard and esteem, but as it was too late to recede, I should now, independently of him (Dr. Wilkin), after seeing the case, do what I thought right, and give an unbiassed opinion upon it.

On arriving at the house, I made the same statement to Mr. C. and other members of the family; and to the remark, that Mr. C. had a strong prejudice against medicine, I replied that, unless my views were in all respects acted upon, I should decline interfering or taking any responsibility in the case. I saw Mr. C., and found him weak and excitable, his nervous system much shaken, with an irritable ulcer on the heel, and sinuses running in various directions. These I laid open, applied a large poultice, recommended the diet (which was a generous one) to be continued, and prescribed bark and nitro-muriatic acid. On adjourning to the drawing-room, I explained that, whatever might be the feeling respecting medicines, or their notion about homœopathy, this was obviously no case for it; and I had a reasonable hope, now that the sinuses were laid open, the generous diet, aided by what had been prescribed, would lead in a short time to a favourable result. Mrs. C. then read me a portion of a letter (the second, I believe, received from Mr. A.), in which, so far as I recollect, it was stated that, being President of the Medico-Ethical Society of Shrewsbury, he could not meet Dr. Wilkin as a homœopathist. I gave my full approval to the sentiments expressed in that letter, and pointed out the impossibility of Mr. A., if he considered Dr. Wilkin to be a homœopathist, meeting him in consultation. I suggested that Mr. A. might meet me, for I neither approved of of nor practised homœopathy. I urged this point so strongly, that I was styled Mr. A.'s advocate.

This suggestion was declined, partly on the ground that this step would involve the dismissal of Dr. Wilkin, in whom, from his kindness and attention, the family had the greatest confidence;—a step, moreover, which they felt assured would not, with the feeling entertained by Mr. A. to him, be submitted to. Dr. Wilkin, I may here remark, was in constant attendance upon Mr. C., slept in the same room with him, was at once nurse and medical attendant, and appeared, so far as I could judge, to be treated as one of the family.

I returned to Liverpool between six and seven the following morning; and, a few days afterwards, fearing, as the time I had spent in the house had been so short, that my views might not be properly understood or carried out, I wrote a letter explaining fully the nature of the case, and urging the necessity of giving the medicine I had ordered; stating that, if the acid disagreed, the chlorate of potash was to be substituted for it; that I feared, unless plenty of support were conjoined with the medicine, we might find purulent deposits occurring elsewhere.

On the 2nd and 3rd of December, so far as my recollection serves me, I received other telegraphic message, stating that matter had formed in the hand. On both occasions, I telegraphed back that I was so circumstanced that I could not possibly attend. On the 4th, I received a most pressing message, urging me to visit Mr. C. without delay. I did so. I found matter in the palm of the hand, on the dorsum, and in the fingers: these I laid open. I found, moreover, an abscess on the lower part of the calf of the leg: this was laid open also. The heel was nearly well: the general symptoms were worse. The case was now clear enough; its nature was fully explained, and its probable issue predicted. I found that Mr. C. had taken the medicine prescribed for a time, but had declined to take more of it. I ordered quinine. I left, as before, early on the following morning (Thursday), having previously agreed, at the request of the family, to visit him on Saturday, and, if necessary, to remain in attendance upon him until Monday morning.

On arriving at the Shrewsbury station on Saturday night, and learning from Dr. Wilkin that Mr. C. was not so well, after ascertaining from Dr. Wilkin that the family would approve of it, I telegraphed to London for Sir B. Brodie, or in case he could not come, to send Mr. Fergusson. This procedure met with the entire approval of the family, to whom I stated that after the unfavourable view I had taken of the case, I could no longer grapple with it single handed. Mr. Fergusson arrived on Sunday night. Matter had now formed in the calf of the leg higher up, and in the thigh, and Mr. C. was obviously sinking. Mr. Fergusson was requested by me to state to the family whatever he had to say upon the case, without either Dr. Wilkin or myself being present. What he did say, or how he said it, I don't know, except that he gave the family to understand that the case was a hopeless one. The rest is soon told. I left as before, between six and seven on the following morning, Mr. Fergusson, I believe, at eleven, and poor Mr. C. died at four p.m. On Sunday I went to Shrewsbury to see Mr. A., but found he had gone into the country to a considerable distance. I left a note stating my regret at not seeing him, informing him at the same time of Mr. C.'s hopeless condition.

On the 12th of December I received a Shrewsbury paper, containing the paragraph quoted by Mr. Humphreys in the Journal of the 10th of January: this was accompanied by a

letter from Dr. Wilkin, in which he stated that he knew nothing of that paragraph until his attention was directed to it: that he trusted I should think little or nothing of having my name associated with homœopathy in my attempt to save life. In concluding his letter, he adds, "Homœopathy I have nothing to do with; as a man, and responsible for my acts and deeds, I do what I conceive right." I fear the foregoing statement may appear unnecessarily and tediously long; but having decided to bring the case before the profession, I have felt anxious to do justice, not only to myself, but to all parties interested in it. Those only can judge of the feeling it has excited in the neighbourhood of Shrewsbury who know the value of the gentleman who is the subject of it, and the esteem in which he was held. I think the feelings of a family who have sustained so great a loss should receive some consideration; and I trust the statement I have made will set the matter at rest. I might, on some points, have entered more fully into particulars, but I have been anxious throughout to do unto others as I would that they should do unto me.

I think, notwithstanding that appearances were so much against me, and that those appearances were strengthened by the statement in the *Shrewsbury Chronicle*, you will now feel satisfied that I neither approve of, sanction, nor practise homœopathy; and that I neither went to meet a homœopathist, nor approved of homœopathy, nor consulted with a homœopathist; in fact, there was no consultation in any sense of the word; and I believe most conscientiously that whatever course Dr. Wilkin may pursue in other cases, that in this case he was not accessory, either directly or indirectly, to its fatal issue.

I am, etc., JAMES LONG.

3, Hardman Street, Liverpool, February 2nd, 1857.

ON THE AMOUNT OF MORTALITY FROM CHLOROFORM.

LETTER FROM JAMES ARNOTT, M.D.

SIR,—There have been published lately, in the BRITISH MEDICAL JOURNAL, two papers on the mortality from chloroform, animadverting upon certain opinions which I have expressed on the subject, and which were inferences from extensive statistical inquiries. I trust you will give me the opportunity of offering some explanation.

The first of these communications contained a return of the mortality amongst the whole of the surgical cases, for certain periods before and after the introduction of etherisation, in St. Bartholomew's and Guy's Hospitals. As there appeared little difference in this respect between the two periods, it is inferred that chloroform could not have affected the mortality after certain operations in the manner I have stated. But I conceive it impossible to form an accurate judgment on the question from such generalities; for chloroform might prove fatal in half the number of capital operations, and yet a decrease of the mortality of the whole number of the surgical cases in these large hospitals be shown, in consequence of improvements in their general management, or in other departments of surgery. The only practicable mode of settling the question is to confine the comparison to the operations specified.

The other paper professes to give a list of the deaths which have occurred during and from the administration of chloroform, or from its immediate effects. As I conceive that these deaths constitute a very small amount compared with those occurring long after the drug has been administered, and in consequence of certain morbid affections to which it has caused a predisposition, I do not attach much importance to such lists; they are chiefly of use as affording indisputable proof of the poisonous nature of the agent. I shall only observe, with respect to that now brought forward, that it differs materially from others which have been published. It shows, for example, that chloroform had proved fatal in only twenty-six cases before October 1852; whereas, in Dr. Crisp's return on the same subject, published in June 1853, the sudden deaths are stated as having then amounted to forty-two.

The framer of this list is of opinion that no deaths from chloroform have been concealed. In this opinion I believe he is singular. The framer of the first part of the list, published in the ASSOCIATION MEDICAL JOURNAL in 1853, expressly states that he "cannot suppose that all the deaths which have occurred have been made public", and allows that the number given by him may on this account be doubled. I have more than once expressed the conviction that much more than half the number have not been published, and given several reasons for thinking so. Though, for obvious reasons, we do not often hear of actual deaths occurring suddenly from chloroform, we hear very frequently of patients being rescued from imminent danger incurred by it. When Dr. Crisp read the paper containing the catalogue just referred to before the Medical Society of London, M. Ricord, as we are informed in the paper, had already saved four patients by the mode of artificial respiration which he invented; and Dr. Marshall Hall relates, in the last number of the *Lancet*, that "three persons, apparently dying from the effects of chloroform," were lately rescued by *his* peculiar method. Although cases of death from chloroform are very rarely published in France, one of the medical academicians, M. Guérin, informs us (*Gazette Médicale*, 1853) that "he believes that consecutive deaths from chloroform are very numerous; and that, on making inquiries of many surgeons both in Paris and in the provinces, most of them have confessed that they have had occasion to regret the occurrence of these accidents." The surgeons of whom he made these inquiries evidently did not consider that they were bound to divulge such unfortunate occurrences, more than the accidents which must always arise from the use of other powerful and highly useful medicines.

It is important, however, to make a distinction, in respect to this publicity, between chloroform and the other anæsthetic vapours of similar character that are now being substituted for it. There is no longer any necessity for distressing the relatives of those cut off by chloroform by divulging such accidents, as we require no further proof of its fatal properties; but, although it is most probable that sulphuric ether, amylene, etc., are equally dangerous if administered so as to produce an equal degree of anæsthesia, it is very important that we should have no doubt on this point, and that every information should be afforded. The system of concealment which has lately, and not improperly, obtained with respect to chloroform, would be highly blamable as regards these substitutes.

The most important question in connexion with the subject of sudden death from chloroform is, whether we are not justified in using it notwithstanding such accidents, on account of the saving of life upon the whole, in consequence of the supposed beneficial influence of chloroform on the results of operations. I believe that the statistics which I have recently published have definitively answered this question. I have brought better proof than can be brought in support of half of our universally received medical opinions, that, instead of there being a saving, there has been an enormous loss of life from the influence of chloroform on operations. Three months have elapsed since this exposition was made, and up to this time not a single fact has been brought in contradiction to it. The same gentleman who has furnished the catalogue of sudden deaths above commented upon has very recently published a return of the deaths from amputation under chloroform at St. George's Hospital, which, instead of opposing, as strongly confirms my views as any such limited document can do. His average mortality of 29·9 must be increased, as he has included the amputations of the forearm, which have been excluded in the other comparative lists; and his preference of other statements of average mortality before the use of chloroform to since cannot be allowed. One of Mr. Phillips' averages was founded on data rejected by the Council of the Medico-Chirurgical Society, and the other makes the mortality from amputation of the arm greater than that of the thigh! M. Malgaigne's statements of the mortality of the Parisian hospitals can furnish no useful comparison with London; for our hospitals have for a very long period formed, in respect to salubrity, and particularly recovery from amputation, a striking contrast to those in Paris.

The table which I constructed, showing the mortality from the principal amputations before etherisation was introduced, was founded on all the reports which had been published of the mortality from amputations in the London hospitals; but the difference between it and the tables of Dr. Simpson and Mr. Phillips is too insignicant to affect the inference. That of Dr. Simpson, showing the highest rate of mortality before chloroform, or 24 per cent., shows also, when compared with the uncontested statement of the mortality since, that there has been an increase of 10 per cent., or that 100 more now die from amputations in every 1000 operated upon than formerly.

I am, etc., JAMES ARNOTT.

Feb. 11th, 1857.

ON THE CONTRACTION OF THE BRONCHI DURING INSPIRATION.

Sir,—I have long delayed replying to Dr. Bell's able communication of Nov. 29th, 1856, in which he advances the original idea of bronchial contraction in inspiration, to which I offer the following as explanation; long expecting that, before I did so, some one might have advanced one better.

During inspiration the capacity of the chest is increased in every direction, therefore the bronchi become elongated (in the same ratio as the capacity of the chest is increased), and contraction must necessarily ensue in proportion to the elongation of the bronchi; and I would suggest that, during the elongation and contraction, the air present in the tubes is drawn along towards the air-cells, which could not take place if the tubes dilated during inspiration. It is not unreasonable to suppose that the involuntary muscular fibres, passing from one extremity of the cartilages to the other, may, by their contraction, assist to lessen the diameter of the tubes.

Dr. Bell considers the prolonged expiration in phthisis to depend in a great measure upon the tissue of the lungs being interspersed with tubercle, thus rendering partially inert the elastic tissue of the bronchi, which we suppose to assist in expiration.

I am also led to notice a few remarks made by Dr. Smith in his Second Lecture, published 17th January (page 47). He says: "We have also yet to learn more in reference to the varying capacity of the bronchi, before we can fully account for the continued reception of the usual quantity of tidal air, after the destruction of so large a portion of the air-cells."

My explanation of Dr. Bell's original remarks, together with the knowledge that the bronchi still remain patulous after destruction of the air-cells, will show why the quantity of tidal air varies so little in phthisis from the healthy state, as we should at first be led to expect. And it is very questionable whether what Dr. Smith asserts be true, that the bronchi serve no better purpose than "iron tubes" do to convey water; for, if the bronchi were inelastic, like "iron tubes", they would prevent the alternate expansion and contraction of the chest.

It is true that, in proportion to the diminished vital capacity or degree of expansion of the chest during inspiration, so would the number of inspirations be correspondingly increased also, shewing the amount of healthy surface destroyed, and *vice versâ*. The practice of wearing the stays tight, or any other means that may hinder the due expansion of the chest, and thus, according to Dr. Smith, the lessened vital capacity of the chest thus acquired, acts as a great cause of the disease; thus causing an increased number of inspiratory acts to become necessary, much more than would occur if the inspirations were of the full depth. Dr. Smith says: "As the air vesicles are endowed with especial nervous power, there would be special liability to disease." It follows that the part most used, or subject to increased inspiratory action (when the vital capacity is lessened), would be the most liable to disease. And, as Dr. Smith says the lower lobes are most used, it would follow that they should become the first affected with the disease, which is not the case: therefore, as the upper lobes are first affected with deposit, it follows that they are the most used.

Dr. Smith also says: "The direction of the bronchi and trachea is downwards; the direction of the expansion of the chest and of the indrawing of the air is downwards; and the greatest degree of expansion of the chest is downwards, or at the base: and hence it would follow that the air would more readily pass from the trachea to the lowest part of the lungs, and those parts would be most readily distended with air."

I submit that the direction of the bronchi is influenced by the shape of the chest; that the amount of breathing surface and tubes exists in a large amount in proportion to its capacity at the apex, as at the base; but, as the lower lobes are larger, so must the vital capacity or expansibility of that part be likewise increased: but this expansibility is only apparently greater at the base, not actually, when compared with the small size of the apex.

The air-cells of the apex might, *à priori*, be expected to be first filled, the distance from the bifurcation of the trachea being least: and, again, the vesicular murmur is most audible at the apex, thus receiving a greater part of the inspiratory force than any other part, when compared to its bulk.

I beg you will insert these remarks, if you consider them deserving. I am, etc., J. R.

DR. FELL'S TREATMENT OF CANCER.

Letter from J. W. Fell, M.D.

Sir,—In your article of February 7th, while you state with much candour and perspicuity the prominent features of my mode of treating cancer, you are mistaken when you assert that I employ no general or constitutional treatment. While I admit that cancer is in many cases a local disease, still for its development there must be a predisposition. To remove this, I invariably employ internal remedies; and to the administration of these I impute much of my success in the treatment of these cases. I am, etc., J. W. Fell.

70, Warwick Square, Belgrave Road, Feb. 10th, 1857.

Parliamentary Intelligence.

HOUSE OF COMMONS.—Wednesday, February 4th.

Mr. E. Ball presented a petition from medical officers, praying for relief from the Poor-Law restrictions.

Thursday, February 5th.

POOR-LAW MEDICAL RELIEF.

Mr. Rice asked the President of the Poor-Law Board if it was his intention to propose, during the present Session, any measure on the subject of medical relief.

Mr. Bouverie said he had no intention of bringing in such a Bill.

POISONOUS DRUGS.

Mr. Brady asked the Secretary of State for the Home Department, if it was the intention of Her Majesty's Government to introduce any measure this Session for the better regulation of the sale of poisonous drugs.

Sir G. Grey said it was his intention to bring in such a Bill; indeed, the Bill had been prepared. [*Cheers.*]

Friday, February 6th.

CRIMINAL LUNATICS.

Mr. Floyer asked the Secretary of State for the Home Department whether it was his intention to take any steps in the present Session of Parliament towards making adequate provision at the public expense for the maintenance and care of criminal lunatics?

Sir G. Grey hoped a beginning might be made in providing such an establishment, but was unable to say whether a vote would be taken in the present Session. Inquiries were being made with regard to the site and the expense.

Medical News.

BIRTHS, MARRIAGES, DEATHS, AND APPOINTMENTS.

** In these lists, an asterisk is prefixed to the names of Members of the Association.*

BIRTHS.

*Colebrooke. On February 9th, at Southborough, Tunbridge Wells, the wife of Henry Colebrooke, M.D., of a daughter.

*Crompton. On February 8th, at Cavendish Place, Manchester, the wife of Samuel Crompton, Esq., Surgeon, of a son.

Day. On February 8th, at 38, Kensington Park Gardens, the wife of Charles Day, Esq., Surgeon, of a son.

Mennie. On December 27th, 1856, at the Fort, Bombay, the wife of John Mennie, Esq., Assistant-Surgeon H.E.I.C.S., of a daughter.

Nourse. On February 9th, at West Cowes, Isle of Wight, the wife of William E. C. Nourse, Esq., of a daughter.

Squire. On February 1st, at Brightlingsea, Essex, the wife of Samuel Nathaniel Squire, Esq., Surgeon, prematurely, of twin sons.

MARRIAGES.

Arnold—Workman. Arnold, the Rev. Dr. John Muehleisen, Chaplain to St. Mary's Hospital, to Maria, daughter of *Thomas Workman, Esq., of 10, Inverness Road, Bayswater, at St. James's, Paddington, on February 10th.

BAILLIE—GRAHAM. BAILLIE, James, M.D., of Brook Street, to Margaret, daughter of the late James Graham, Esq., of Lancefield, Glasgow, at St. George's, Hanover Square, on February 4th.

BIANCHI—DOUBLEDAY. BIANCHI, Robert, Esq., Surgeon, of 43, Stamford Street, to Catherine, only daughter of the late John Doubleday, Esq., of Long Clawson, Leicestershire, and niece of *Edward Doubleday, Esq., Surgeon, Southwark.

KEYS—KEMP. KEYS, George Francis, Esq., Surgeon, of Warwick Street, Regent Street, to Caroline, widow of John Kemp, Esq., late of H.M. Inland Revenue Office, and Acton, Middlesex, at St. Mary's, North End, Fulham, on Jan. 31st.

DEATHS.

COULL. On January 27th, at Elgin, Mrs. Coull, widow of the late James Coull, M.D., of Ashgrove.

SEAMAN. On January 29th, at Bradford, Yorkshire, aged 69, Mary Ann, widow of the late Charles Seaman, Esq., Surgeon, of London.

WATSON, James C., M.D., H.E.I.C.S., Bengal Presidency, in London, aged 34, on January 27th.

APPOINTMENTS.

*PROPERT, John, Esq., has been appointed High Sheriff for Cardiganshire.

SANDWITH, Humphry, M.D., late of Kars, has been appointed Colonial Secretary of the Mauritius, in the room of Mr. C. J. Bayley, now Governor of the Bahamas.

VELPEAU, M., the celebrated Parisian Surgeon, has been elected a Honorary Member of the Royal Medical and Chirurgical Society.

PASS LISTS.

ROYAL COLLEGE OF SURGEONS. MEMBERS admitted at the meeting of the Court of Examiners on Friday, February 6th, 1857:—

ASKHAM, Henry Francis, Eckington, Derbyshire
BRADSHAW, Alexander Fredk., Bishopsgate Street Within
FULLER, Charles Chinner, Osnaburg Street, Regent's Park
HUNTLEY, William Albert, Brixton
HUTCHESON, Robert William, Eleuthera, Bahamas, West Indies
MACKENZIE, John Thomas, Toronto, Canada West
MASON, Benjamin Earnshaw, Calcutta
MEREDYTH, Adolphe Aschele William Leodore Colominti, Angers
NOYES, Alfred William Finch, Stokesbay, Gosport, Hants
SCOTT, Walter, Tasmania
SKINNER, Frederick, Hendon, Middlesex
TIMES, Henry Gorsuch, Thayer Street, Manchester Square

At the same meeting of the Court—

EVANS, Richard, passed his examination for Naval Surgeon. This gentleman had previously been admitted a member of the College: his diploma bearing date the 20th of February, 1850.

HEALTH OF LONDON:—WEEK ENDING FEBRUARY 7TH, 1857.

[*From the Registrar-General's Report.*]

THE deaths registered in London, which had been in the first week of January 1135, and in the last week of the same month 1209, rose in the week that ended last Saturday to 1368. In the years 1847-56, the average number of deaths in the weeks corresponding with last week was 1180. If this average is raised for the purpose of comparison, proportionally to increase of population, it will become 1298; and hence it appears that the rate of mortality which prevailed last week was higher than the average rate in the beginning of February.

The increase, which the present return shows, on the deaths in January, arises from the recent depression of temperature, which commenced towards the end of that month, and continued till near the end of last week. The weather was generally mild in the first three weeks of the year; the mean temperature was 37°; in the fourth week it fell to 31·2°, and last week it was 32·3°; and on two days since the 28th ult. it has been 12° below the average.

The deaths from diseases of the respiratory organs, the class which more than others rises and falls with the fluctuations of heat and cold, rose last week to 359; they had been for some previous weeks about 280. The average of this class, corrected for increase of population, for ten weeks corresponding with last week, is 285. The deaths from bronchitis in the last four weeks were 170, 164, 166, and 224. The increase of pneumonia in the last week is much less considerable. Phthisis, which was fatal in the previous week in 146 cases, numbered last week 184. Hooping-cough carried off 61 children; and as regards the extent to which it was fatal, showed little change on recent weeks. A child died of hooping-cough at 2, Church-court, Westminster, being the fourth fatal case of this complaint in the same house. Four children died of measles in the Marylebone workhouse; the total number of deaths from this disease was 38. Only 3 deaths from small-pox were registered last week in London.

The deaths of four nonagenarians were returned; the two oldest of these aged persons, namely, a widow in Queen-street, Ratcliffe, and a lady in Southampton-row, had attained the age of 96 years.

Last week the births of 976 boys and 915 girls, in all 1891 children, were registered in London. In the ten corresponding weeks of the years 1847-56 the average number was 1620.

At the Royal Observatory, Greenwich, the mean height of the barometer in the week was 29·762 in. The lowest reading in the week was 29·41 in.; the highest occurred on Wednesday, and was 30·14 in. The mean temperature of the week was 32·3°, which is 5·7° below the average of the same week in 43 years. The mean temperature of Sunday (the 1st), was 24·5°, which is 12·7° below the average of the same day; the *lowest* temperature on Sunday, which was also the lowest in the week, was 20°. The mean daily temperature continued below the average till Friday, on which day it was 42·2°. The *highest* reading in the week was obtained on Friday, and was 47·7°. The range of temperature in the week was 27·7°. The mean dew-point temperature was 29°, and the difference between this and the mean air temperature was 3·3°. The wind blew at the beginning of the week generally from the south-east, on Wednesday from the north-east, and on the last three days from the south-west. Scarcely any rain fell; on Monday and Wednesday there was snow, but of small amount.

MUNIFICENT BEQUESTS. We stated some time ago that the late Sir Hugh Richard Hoare, of Stourhead, Wilts, and Lillingstone Lovell, Bucks, who died on the 10th ult., upon entering his 70th year, in November last, presented £1,000 to the Bucks Infirmary. We have now the pleasure to state that he has, by his will, made the following munificent bequests, viz.; To the poor of Stourton, Wilts, £2,000; Westminster Hospital, £2,000; St. George's Hospital, £1,000; Middlesex Hospital, £1,000; Cancer Hospital, £1,000; Consumption Hospital, £500; Medical College, £500; Sarum Infirmary, £500; Wilts Friendly Society, £500; Society for the Propagation of the Gospel, £500; Corporation of the Sons of the Clergy, £500—making a total of £11,000. (*Oxford Journal.*)

TO CORRESPONDENTS.

LETTERS and other COMMUNICATIONS for the JOURNAL should be directed to the private address of the Editor, 39, Curzon Street, May Fair.

To CONTRIBUTORS. The Editor would feel glad if Members of the Association and others, would cooperate with him in establishing as a rule, that in future no paper for publication shall exceed two pages of the Journal in length. If the writers of long communications knew as well as the Editor does, that lengthy papers *always* deter the reader from commencing them, this great evil would never arise. Brevity is the soul of medical writing—still more than of wit.

Members should remember that corrections for the current week's JOURNAL should not arrive later than Wednesday.

NOTICE.—DR. WYNTER will feel obliged if the Associates will address all Post Office Orders in payment of Subscriptions, to the Publisher, Mr. HONEYMAN; and he would also feel obliged by their sending all communications respecting the non-receipt of the Journal, to the same address; as both these matters are out of the province of the Editor.

LONGSIGHT. The Royal College of Surgeons of England has no power to prevent a member of the Royal College of Surgeons of Dublin, or any other College, from practising in England.

MR. BARBER'S Letter shall appear next week.

Communications have been received from:—DR. JAMES ARNOTT; DR. HAYES JACKSON; MR. T. HOLMES; MR. CHARLES COATES; MR. WALTER JOHN BRYANT; MR. JOSEPH GODDEN: MR. G. REEDAL; MR. JAMES JAMES; SIR CHARLES HASTINGS; DR. BOWER HARRISON; SIR HENRY COOPER; DR. G. WEBSTER; MR. THOMAS NUNNELEY; DR. G. G. ROGERS; J. R.; MR. A. T. H. WATERS; DR. W O. MARKHAM; MR. R. J. METCALFE; MR. HENRY DUNCALFE; DR. DAVID NELSON; MR. J. V. SOLOMON; MR. GEORGE ROSS; DR. FELL; MR. MCDERMOT; MR. EDWARD BARBER; MR. LANCELOT NEWTON; DR. J. R. NICHOLSON; SIR JOHN FORBES; MR. JAMES LONG; DR. RADCLYFFE HALL; MR. JAMES SCHOLFIELD; MR. WESTALL (with enclosures); and DR. EDWARD SMITH.

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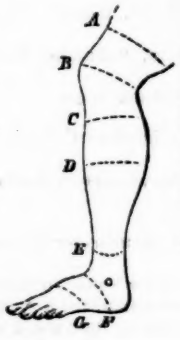

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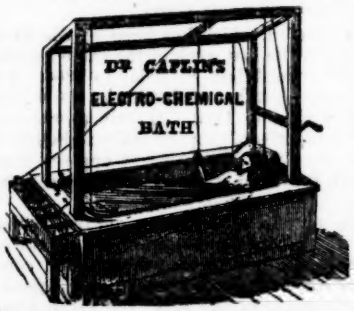

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